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TABLE OF CONTENTS

F. KUHLMANN. A New Method of Weighting and Scaling Mental Tests 181
R. H. ELWOOD. The Rôle of Personality Traits in Selecting a Career: The Nurse and the College Girl 199
HARVEY C. LEHMAN AND PAUL A. WITTY. The Compensatory Function of the Sunday Funny Paper 202
DOUGLAS FRYER. Predicting Ability from Interests 212
SAMUEL RENSHAW. An Experiment on the Learning of "Paired Associates" 226
NOTES AND NEWS 234
BOOK REVIEWS 236
NEW BOOKS AND PAMPHLETS 249

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A MEDIAN MENTAL AGE METHOD OF WEIGHTING AND SCALING MENTAL TESTS[1]

F. KUHLMANN

Director, Division of Research, Minnesota State Department of Public Institutions

A good deal has been said about the advantages of weighting tests, so as to make each test item count in the final score in proportion to the value of that item in determining mental development. None of those in common use have employed any weighting other than that given in the "raw score," wherein each test or item counts for the same as every other item, and which is usually termed "no weighting." There has been no conclusive evidence that for the tests in use the different methods of weighting that have been suggested by the writers on statistical methods result in any improvement in the validity or reliability of the test results. This is undoubtedly due to the combined effect of insufficient data, inadequate methods of proof, and tests in which the different items were already roughly of the same value, and were thus correctly weighted by the raw score method.

The present article describes a different procedure in weighting tests and in scale construction. The chief thing in the method consists simply of using an age norm for each trial in each test in the group-test battery, instead of only for the

[1] The median mental age method wherein a subject is scored in terms of a mental age on each test in a battery, and the median of the several mental ages made on the several tests then taken for the subject's mental age on the battery was first suggested by Pintner and Paterson, "A Scale of Performance Tests," D. Appleton and Company, 1917. They do not follow quite the same procedure in standardization, and do not point out what we think are several of the chief advantages of the method, but the main idea is the same.

summation or total score on the battery. Instead of determining the average total score that children at each age make on the battery of tests, we have determined first the average age of children that pass just one trial, just two trials, etc., of each test. It is the several advantages that this latter procedure makes possible, and the principles involved, that I wish to present. To do this it will be helpful to give a brief summary of the steps that led up to this last change.

The present group tests had their origin in 1916, when we first began to use them in surveying public schools to find the subnormals.[2] The first form consisted of a Binet scale adapted to group work. The individual tests in the scale were assigned to ages, a child was scored on each test as passed or not passed, and his mental age was computed as on the original Binet scale.[3] The scale had two noteworthy advantages. First, it required careful fitting of each test to a given age, and thereby gave it a known and more or less definite value or weighting. Second, the mental age obtained was free from the unreliability associated with a total or average score for a small number of tests in a battery. Its chief disadvantage was in the requirement of too many age-groups of tests in the battery to cover all the mental levels likely to be found in any group of children tested at a time, and shortening the scale meant incomplete results for the dullest or brightest in the group. Evidently,

[2] Various assistants in the Research Division have had a share in the development of the present group test scale, which has been changed and entirely reorganized many times. To be mentioned especially, in chronological order are Dr. Frances Lowell, Dr. Maud Merrill, Charlotte Lowe, and Dr. Rose G. Anderson. Of these Dr. Anderson had by far the major share, and is responsible for designing and selecting most of the tests now in the scale, for writing most of the directions for giving the tests, and for more of the statistical work involved than was done by any other. The writer's share has been largely that of general supervisor, determining the principles and methods to be followed in selecting tests, and designing scales, and is responsible for the present method of weighting and scaling the tests.

[3] See: Lowell, Frances. A group intelligence scale for primary grades. Journal of Applied Psychology, September, 1919, vol. iii, pp. 215–247.

giving up the Binet "all or none" method of scoring each test, and allowing credits in proportion to the total number of items passed, as is done in group tests now, would do away with this short-coming.

In all subsequent forms of the tests we adhered to the plan of definitely fitting each test to a given age. It was aimed to so adjust each test that half its items or trials would be passed by about two-thirds of the children of the age to which the test was assigned. Tests were sought for each age that would give the largest possible percentage increase in the number of children of each successive higher age that would pass this two-thirds of the trials of the test. This resulted not in one battery of tests for several ages, as is the custom with other group tests, but in a sliding scale of group tests, with a partly different battery for each age. For example, for eight-year-old children the easier tests that were used for seven-year-olds were dropped out, and more difficult tests, not appearing in the battery for seven-year-olds, were added, this process being repeated for each age.

In the second form of the tests the unweighted raw score method was used. The average total number of trials in the tests in a battery was determined for each age and these totals were used as the age norms, as is done now in other group tests. This has a number of disadvantages, especially when combined with some of the other features which it was thought essential to retain. One of these other features concerns a general rule in regard to the optimum number of trials for a test. That number should, on the whole, be smaller the younger the children, for young children lack sustained effort applied to a uniform task, and we do not want every test in a battery to measure this same function. Fitting each test to a given age as described meant making them unequal in difficulty and therefore made their respective contributions to the total score on a battery of tests of unequal value. Again, fitting each test to an age increased the liability of zero scores, no trials passed, and maximum scores, all trials of a test passed. Both zero and maximum scores cease to measure accurately,

and zero scores, in their effect on the total score for the battery of tests, are especially detrimental to reliability. It is relatively easy to avoid zero and maximum scores, but it is done at the sacrifice of something more important. One might make each test so easy that even the dullest or youngest in the group would always pass some of the trials, and then have enough trials in each test or have the time allowed short enough so that the oldest and brightest in the group could never pass all trials. Such a battery of tests would still give age norms of increasing values with successive ages and on the surface would appear as reliable and satisfactory as any. As a matter of fact, however, each test in the battery would be largely reduced to a measure of one and the same function, the ability to make speed in a relatively mechanical activity.

Finally, in order that the raw scores may give approximately correct weighting the tests must be of such a nature that roughly the same number of trials will be passed in each test in the battery. In ours, some tests in a battery have as few as five trials, and others as many as fifty trials. When all were adjusted so that on the whole half the trials in each test would be passed by the average child one test might contribute only two or three points to the total score, while the other would contribute twenty-five points, that is, would be weighted ten times as heavily. To make all the tests in a battery so that on the whole an equal number of trials will be passed in each by the average child puts an unnecessary and very undesirable limitation on the selection of tests to be used.

In the third form of the tests the raw scores, trials passed, were weighted on the basis of the standard deviation. For each age the percentages of children that passed one trial and no more, two trials and no more, etc., were found and the corresponding sigma scores were substituted for successive number of trials, as given in Rugg's Table V.[4] This weights the scores according to difficulty, assuming that the frequencies of different grades of intelligence are distributed in accordance

[4] See: Rugg, H. O. Statistical Methods Applied to Education. Houghton Mifflin Co., 1907, p. 392ff.

with the characteristics of the normal distribution curve. Age norms were then found for these weighted scores, as before. The difficulties found with the tests in this form are on the whole the same as those found in the next form in which the tests were organized.

In the fourth form of the tests the scores were weighted according to both difficulty and reliability. Correlations were computed between a series of scores on each individual test in a battery with the series of the total scores for all the tests in that battery, these correlations being used as measures of reliability of the individual tests. The sigma scores previously determined were then multiplied by these correlations, which we may call the sigma $\times$ r method, and age norms determined for the scores weighted in this way.

A large number of children in the public schools, and adults in state institutions had been group tested and also examined individually with the writer's revision of the Binet tests given in his "Handbook of Mental Tests." Correlations were computed between the intelligence quotients obtained in the individual examinations and those obtained on the group tests, when the latter were scored in each of four different ways. These four ways were as follows: (1) Using the raw scores, the age norms being the total number of trials passed in the battery of tests at each age. (2) Weighting the trials in each test so that the maximum score, all trials passed, would count for fifteen, and smaller number of trials counting in proportion. For example, in a test with five trials each trial counted three points, while in a test with thirty trials, two trials counted one point. (3) Sigma scoring. (4) Sigma $\times$ r scoring. The correlations found were a few points higher for the last method than for any of the other methods. However, Miss Anderson, who later subjected this method to a most thoroughgoing statistical study in comparison with other methods of scoring the tests, found no significant difference in favor of this last method.[5] She used the total score of each child as given directly

[5] See: Anderson, Rose G. A critical examination of test-scoring methods. Archiv. Psychol., No. 80, 1925.

by the method of weighting the number of trials passed in the test, not the mental ages or intelligence quotients into which these scores might be transmuted.

The reason or reasons why none of the different methods of weighting show any superiority over any other do not appear in any study made so far. The following are some suggestions, which at least reveal some of the complexity of the problem. When the raw scores, number of trials passed, are transmuted into sigma scores, or scores based on any other measure of variability, in order to weight them according to degree of difficulty, these sigma units are units for the age in question only, and they are usually based, as was the case in this study, on the results of children whose exact ages range over a whole year. This range of a year in the exact ages changes the character of the distribution of the scores, by flattening out both extremes of the distribution curve. It cannot give a normal distribution, which is an assumption underlying the method. This flattening out of the extremes of the distribution curve by itself results in an underweighting for the low sigma scores and an overweighting for the high sigma scores. But, since the lower grades of intelligence are eliminated from the schools, the underweighting at the lower extreme may not appear. Our results show such under and over-weighting at the extremes. In practically all the tests under discussion here the successive trials in a test were of the same nature, and therefore presumably of approximately equal difficulty, however different in difficulty the tests as a whole might be. If the sigma scores are true units these scores should then increase in proportion to the number of trials in a test that they represent. Graphs with sigma scores plotted against number of trials should be straight lines. We find to the contrary a universal tendency for the sigma scores to increase more rapidly for the larger number of trials than they should. In figure 1, the total sigma scores, multiplied by ten, on sixteen tests for a thirteen year battery are given on the left and the number of trials at the bottom. If in this graph the lower portion were projected upward in a straight line, as indicated by the dotted line,

instead of in a curve as it is, it would be about one hundred points lower at the tenth trial. This is the equivalent of about three years in mental age at this point.[6] This tendency was present in our tests at each age, and remains when the weighting of the raw scores is changed from sigma scores to sigma $\times$ r. Again, our present method of scoring our tests lowers the high I.Q.'s, and raises the low I.Q.'s as compared with the sigma method of weighting and scoring the same tests. Results of individual examinations also show the same tendency to raise the low I.Q.'s on the group tests. Cases with

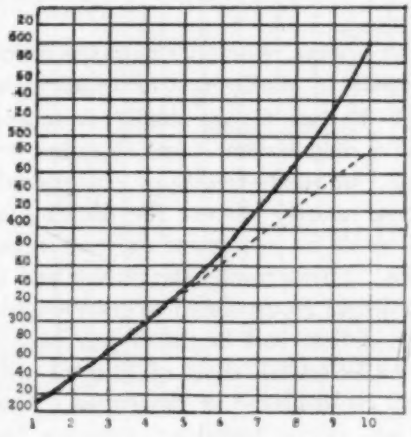


FIG. 1

high I.Q.'s on the group test were not given individual examinations. In other words, when the exact ages of the children for whom sigma scores are computed range over a whole year, say from nine and a half to ten and a half, for example, these sigma scores are not units even for the exactly ten-year-olds, and the error thus introduced is considerable.

Secondly, unreliability in sigma weighted test scores results from the fact that sigma scores for any given age cannot accurately represent grades of intelligence at other ages. If we

[6] See Anderson, op. cit., p. 15, for table on which this graph is based.

are dealing with sigma scores based on ten-year-olds the same score, say 3.0 sigma, will, of course, represent grades of intelligence higher than the ten-year-old 3.0 sigma for all ages below ten, and grades of intelligence lower than this for all ages above ten. Even when the use of a battery of tests whose scores are thus weighted is limited to a single school grade whose average age is the same as the age for which the test scores are weighted, a range of several years in the ages of the children will be covered. Thus the raw scores the older children in the group make will be over-weighted, and the raw scores the younger children make will be underweighted. The resulting error is again considerable.

Third, we must always remember that the number of children at either extreme of intelligence, on which sigma scores are based is likely to be lamentably small. A school population of 1000 would give only several cases at either extreme even in the first grade. A single family of exceptionally bright or dull children may entirely distort the distribution curve at that point, and result in correspondingly erroneous weighting of raw test scores. Furthermore, the dullest and the brightest found in the particular group tested to get the age norms must be taken for the dullest and brightest possible. If at either extreme the cases found fall short of this the whole series of sigma values will be displaced in proportion. One would not think of using age norms based on averages from similarly small numbers of cases.

There is a fourth source of unreliability which we believe entirely outweighs all others in importance. It affects the individual scores independently of the method of weighting raw scores that is used. This is the exceptionally high or low score or two a child may make in a test or two in the battery. This affects accuracy of the age norms as well as reliability of the individual score. It affects accuracy of the age norms when the latter are the averages of total scores of children at each age. Since both zero and maximum scores on some of the tests in a battery lower the average total score used as the age norm, all children who thereafter make neither zero or

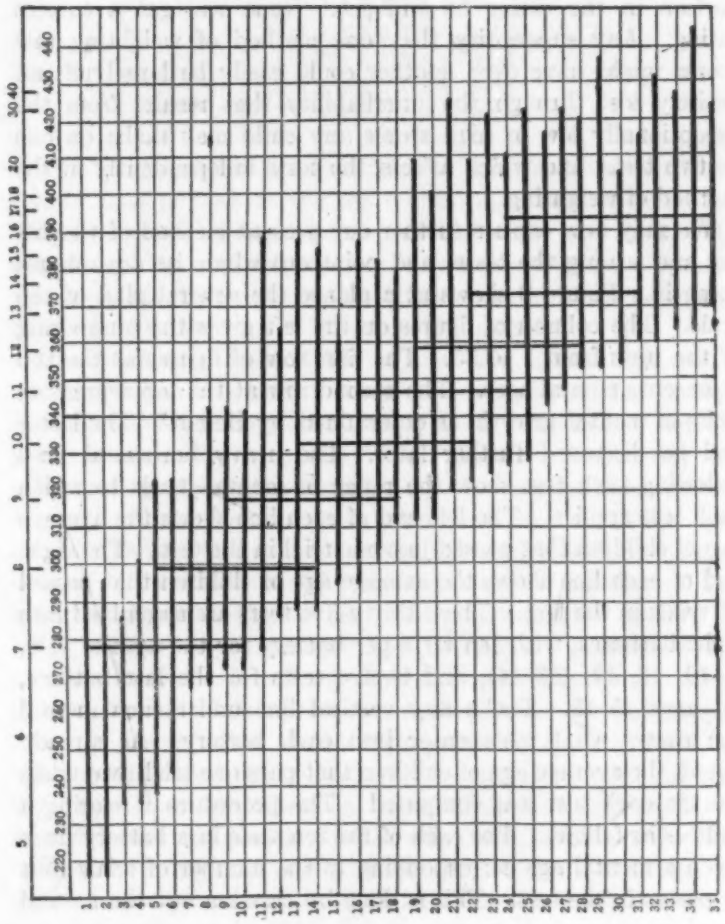


FIG. 2

maximum scores will be rated too high on the tests, and this will be true of the majority. The child, however, who does make a zero score or two will be penalized quite out of proportion in the rating he will get. None will get a correct rating. Any superiority that one method of weighting raw scores might have over another could easily be largely if not entirely lost through the unreliability that results from the exceptionally low or high scores any child may make on one or two tests, and which affects the tests independently of the method of weighting.

We may now explain further our present method of weighting and scaling the tests, and point out where its advantages come in. Figure 2 shows at a glance the general plan of the scale. The column of figures on the left gives the numbering of the tests from 1 to 35. The first row of figures at the top represents mental ages. The second row at the top represents units of mental growth, as determined by Heinis.[7] The latter will be discussed further later. The heavy horizontal lines following each test show the range of mental levels to which each test applies. The left end of each line shows the average age of children that passed just one trial in the test. The right end of each line shows the average age of children that passed all trials in the test. These thirty-five tests are assembled into eight batteries, with ten tests per battery for the ages of 7, 8, 9, 10, 11, 12, (13–14), and twelve tests for the last battery, for ages 15–18. The heavy vertical line under each mental age shows what tests enter into each battery. As already noted, the average age of children that pass one trial, two trials etc., in each test was computed. The procedure in scoring a child is as follows. For each of the ten tests in a battery he is given a mental age corresponding to the number of trials that he passes in the test. This is done by crossing out the mental

[7] Heinis, H. A personal constant. Jour. Educat. Psych., April, 1926. Heinis gives the mathematical formula for a mental growth curve of average, normal children. I have computed the mental growth units for each age from his formula.

ages in the following table of mental ages, given in years and months, and which gives an illustration.

5....1 2 3 4 5 ~~6~~ 7 8 9 10 11
6....1 ~~2~~ 3 4 5 6 7 ~~8~~ 9 10 11
7....1 2 ~~3~~ 4 ~~5~~ 6 7 8 9 10 ~~11~~
8....1 2 3 ~~4~~ ~~5~~ 6 7 8 9 10 11
9....1 2 3 4 5 6 7 8 ~~9~~ 10 11

The blanks we use give the mental ages from four to forty years. The median of the ten mental ages is then taken for the child's mental age score. On the table this is readily determined by counting the five from the top that are crossed out, and then half ways to the sixth. In the illustration, the child has a mental age of 7 years and 8 months.

It was seen in figure 2 above that the mental age norms assigned to the last trials in tests, all trials passed, run as high as fifty years. These high "mental ages" were not, of course, determined in the usual way. We have no empirically established age norms beyond eighteen, the age for High School seniors. The raw scores on the tests increased up to this age, inclusive, either because of a continued mental development or because of a continued elimination from the schools of the lower levels of intelligence in passing through the grades, or both. There has never been any satisfactory method of expressing mental levels above that of the average adult. Our present method of doing this is based on Heinis' mental growth curve. It was found that the graph resulting from plotting his mental growth units against the successive trials of our tests was apparently always a straight line up to age eighteen. We have assumed that it continues in a straight line for levels of mental development above this point. With this assumption, we could assign correct total Heinis' units to each trial in a test above the trials with an age norm of eighteen years and transmute these into mental ages. Figure 3 gives illustrations for tests number 29 and 32. The first part of the graph for each test plots the Heinis units against the trials in the test as actually found. The average age of the children that passed

just one, or two, etc., trials of the test having been found, the Heinis mental growth curve units were substituted for the ages. The dotted part in each graph shows the extension and corresponding Heinis units, which may then be transmuted back into "mental ages." Heinis' formula for the normal mental growth curve is

$$y = b\left(1 - e^{-\frac{x}{d}}\right)$$

where y = total number of units at given age, x = age, b and d = constants with values of 429 and 6.675 respectively, and

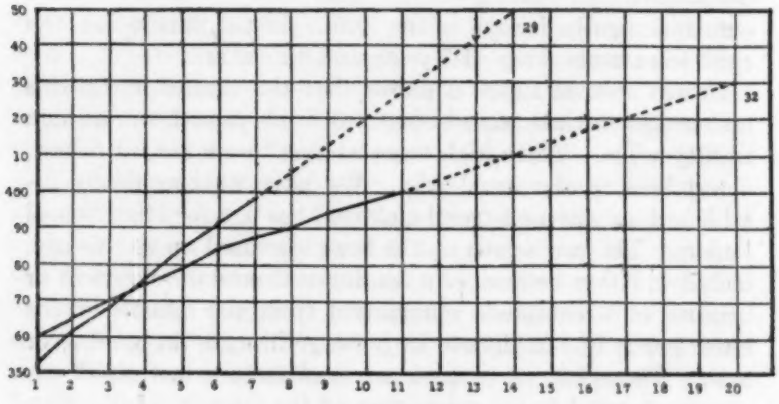


FIG. 3

e = the base of the natural logarithm, 2.71828. From this formula the writer computed the table of values of y at the different ages (table 1). Space will not be taken here to discuss the merits of Heinis' mental growth curve, except to call attention to two facts concerning it. These are, first, this growth curve is exactly what is required in order to give the decreases, with increasing age, in the I.Q.'s of sub-average children, and the increases in the I.Q.'s of super-average children as found by re-examinations by the writer and others. Second, the units in this growth curve are units to the same degree as are the successive trials in our tests on the whole,

TABLE 1

Heinis' Mental Growth Units

	MONTHS											
	0	1	2	3	4	5	6	7	8	9	10	11
1	59	64	69	74	78	82	86	90	94	98	103	107
2	111	115	119	123	127	131	134	138	142	145	148	152
3	155	159	162	165	169	172	175	178	181	184	187	190
4	193	197	200	202	205	208	211	214	216	218	221	224
5	226	229	231	233	236	238	241	243	245	247	250	252
6	254	256	258	260	263	265	267	269	271	273	275	277
7	278	280	282	284	286	288	290	292	293	294	296	297
8	299	301	303	304	306	308	310	311	312	313	314	316
9	318	319	320	322	323	325	326	327	328	329	330	332
10	333	334	335	337	338	339	341	342	342	343	345	346
11	347	348	348	349	350	351	352	353	354	355	356	357
12	358	359	360	360	361	362	363	364	365	365	366	367
13	368	369	369	370	371	372	372	373	374	374	375	376
14	376	377	378	378	379	379	379	380	381	382	382	383
15	384	384	385	385	386	387	387	388	388	388	389	389
16	390	390	390	391	392	392	393	393	394	394	395	395
17	396	396	396	397	397	398	398	398	399	399	399	400
18	400	400	401	401	402	402	402	402	403	403	404	404
19	404	404	405	405	405	406	406	406	407	407	407	407
20	408	408	408	408	409	409	409	409	410	410	410	410
21	411	411	411	412	412	412	412	412	412	413	413	413
22	413	413	413	414	414	414	414	414	414	415	415	415
23	415	415	415	415	416	416	416	416	416	417	417	417
24	417	417	417	418	418	418	418	418	418	419	419	419
25	419	419	419	419	420	420	420	420	420	420	420	420
26	421	421	421	421	421	421	421	421	422	422	422	422
27	422	422	422	422	422	423	423	423	423	423	423	423
28	423	423	423	423	423	423	423	423	424	424	424	424

29–0 to 30–6,	424
30–7 to 32–5,	425
32–6 to 35–0,	426
35–1 to 38–7,	427
38–8 to over 60	428

since the regression of one on the other is rectilinear. We are using it here only as method of assigning "mental ages" to raw scores that represent mental levels above eighteen years and this is necessary, of course, only for the tests that are used to measure mental levels above the average for this age.

Several comparisons have been made between our present method of weighting and scoring and other methods. In each instance the results have been in favor of our present method. In one school 71 cases in grades II to VI were given individual examinations with the tests in my Handbook, immediately following the group tests. All in this group had fallen below an I.Q. of 0.85 on the group tests. The group tests were then scored by a second method which corresponds to the method of other group tests in so far as the influence of zero, maximum and other exceptionally low or high scores on particular tests in the battey is concerned. Remember that in our present method, which we will call the "median mental age" method, we have age norms for each trial in each test. A child is then given a mental age for each test on which he passes any trials. The median of the ten mental ages for the battery of tests is then taken for his mental age. The second method of scoring the group tests just mentioned used the average of these ten mental ages for his mental age instead of the median. We will call this the "average mental age" method. The results of the individual examinations were then compared with the group test results, when scored by the two methods. The difference in mental ages for the individual and group test when the latter were scored by the median mental age method was less than when scored by the average mental age method in 69 per cent, and the same in 3 per cent of the cases. The correlation (Pearson product-moment) between the mental ages in months for the individual examinations and the group test mental ages by the median mental age method was 0.87. By the average mental age method the correlation was 0.84.

In a second school 80 cases in grades I and III were group tested. The group test battery was then divided into two halves, including the odd numbered tests in the first half, and

the even numbered tests in the second half. The mental age for each child was then found on each half separately, both by the median mental age method, and by the average mental age method. The average difference in mental ages for the two halves was 13.18 months with the median mental age method, and 17.89 months with the average mental age method, an increase of 35 per cent. Correlations between the mental ages for the two halves were 0.85, for the median mental age method and 0.77 for the average mental age method.

In the same school 100 cases in grades IV to VI were group tested. The average difference between the two halves of the scale was 12.22 months for the median mental age method, and 15.72 months for the average mental age method, an increase of 29 per cent.

It will be noted that when the battery of ten tests is divided into two halves one point of advantage in our present method is lost, in that it leaves an odd number of tests, and the median is then the score on some one particular test. When there is an even number of tests in the battery the median is the middle point between two tests, and two tests enter to determine it. Consequently, the above 100 cases were scored in still another way. This time the first half of the scale included the odd numbered tests, plus number 6. The second half included the even numbered tests, plus number 5. The average difference between the two halves was now 7.62 months, for the median mental age method, and 12.47 months for the average mental age method of scoring, an increase of 64 per cent. The correlations between mental ages on the two halves in this instance were 0.88 for the median mental age method, and 0.78 for the average mental age method.

The above are samples selected from data we happen to have accumulated in the routine work of the department. No special studies were made to compare our group tests with others. In such a comparison there would be two general points of difference. First, the difference in the tests themselves, and second, the difference in weighting and scoring with which this paper deals. The tests themselves, it should be

remembered, are the product of ten years of careful selection and development, guided by the results of about 25,000 group tests given to public school children of all grades, and to adults in penal and other state institutions, by the experienced mental examiners of the department. One instance may be given of a comparison with other group tests. In one school 75 children in grades III to V had been given the Haggerty Delta 2 group tests, and soon afterwards were given our group tests. The correlation between the mental ages, in months, on the two

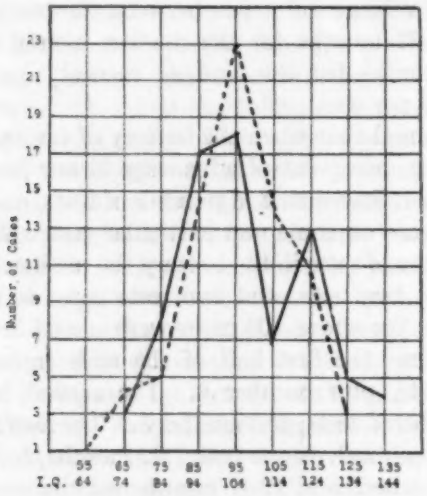


FIG. 4

was 0.81. The average I.Q. on the Delta 2 tests was 98.12, and on ours 99.05. There was, however, a marked difference in the distribution of the I.Q.'s for the two scales, in the following graph, the dotted curve is for our tests, the other is for Delta 2.

The following are, we believe, noteworthy outstanding traits of the tests with the present method of weighting and scoring.

1. The determination of age-norms for each separate test and trial, instead of only for the total score on the battery of

tests, shows exactly where each test applies, its range of application and its discriminative capacity. Without this, much in a battery may be "dead timber" without our knowing it. Poor or worthless tests in a battery lower reliability of the total result, not only by virtually reducing the number of real tests in the battery, but by contributing something to the total score that distorts what might otherwise be fairly correct.

2. Using the median score a child makes on the battery of tests, instead of the total score, eliminates probably the chief source of unreliability of group test results. This median is further improved by making it a median of an even number of tests, ten or twelve. The median score of an uneven number might be more or less displaced in individual cases. In the median of an even number two tests scores enter to determine the exact median used. The unreliability that is eliminated is of course, that resulting from distorted total or average scores due to zero and maximum scores, or otherwise exceptionally low or high scores on a test or two in the battery.

This method eliminates at the same time the inaccuracy of age norms based on averages, since these averages are at times seriously influenced by zero and maximum scores that have entered.

3. This method of weighting the raw scores, trials of each test, avoids the several sources of error and unreliability associated with the sigma weighting, or weighting by other measures of variability, as discussed above. There is no underweighting when only a few trials in a test are passed, and no overweighting when many trials are passed. Also, the age norm for each trial is an average, unaffected by zeros and maxima, is based on a large number of cases, and not determined by a scattered few cases, as at the extremes of a distribution curve.

4. Each test is carefully fitted to the age at which it applies best. Zero and maximum scores are avoided, not by having a large number of trials or by a short time allowed for the performance of the test, and thus partly reducing them all to a measure of speed in a relatively mechanical activity, but by

changing the tests altogether as we progress to higher ages. The whole scale includes thirty-five individual tests, instead of the usual six to eight, used for a range of several school grades.

5. The recommended ten or twelve tests per battery as we now use them may be changed to a smaller or larger number without any other change in procedure. Also, any old test may be dropped permanently from a battery, or new test added permanently, without requiring any further change.

6. A step in getting the score for a child is eliminated. No totaling of the scores of the several tests in the battery is required, and only one table in place of two is used in getting the mental age. In a scale using weighted scores, one table is needed to get the weighting for the raw score, these weighted scores must be totaled, and another table must be used to give the age norm or mental age. For these three steps the procedure in our tests calls for one table giving age norms, or mental ages, for the raw score on each test, and finding the median of the ten or twelve mental ages.

7. The scale of tests is adapted for individual examination as well as for group testing. Any section in the series of the thirty-five tests out of which the eight group test batteries are built up may be used in examining an individual, and as many or as few tests may be used as time or judgment dictates. The median of the mental ages earned on the tests that are used is his mental age. The ages to which each test is normally assigned in the group test batteries may be used as a guide in determining at what point in the series of thirty-five tests to begin the examination.

8. If all tests, group and individual, were weighted and scaled by our present method they would be at once directly comparable with each other, and tests from different scales could be variously combined for special occasions and requirements, without modification or re-standardization.

THE RÔLE OF PERSONALITY TRAITS IN SELECTING A CAREER

THE NURSE AND THE COLLEGE GIRL

ROBERT H. ELWOOD

Colgate University Psychological Laboratory

Young women entering a liberal arts college or a nurses' training school start out on widely divergent paths. They follow courses which lead to sharply contrasting futures. And yet, in spite of these differences, they have had practically the same educational preparation for their advanced studies. Certain definite mental factors undoubtedly govern the choice of a career. These factors are either intellectual, emotional, or a combination of both. Oftentimes, as psychoanalysts have found in individual cases, the determining force is emotional rather than intellectual. The choice, however intellectualized and explained, may be but a subtle expression of a deep-lying personality trend.

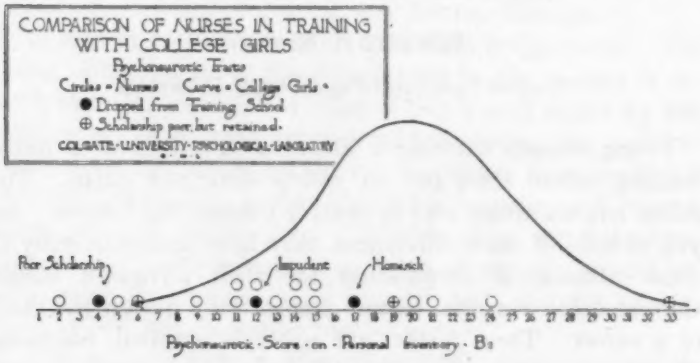
Mental test scores of a group of nurses entering a New York City training school were obtained through the coöperation of Dr. Alice E. Paulsen of the Psychological Corporation. The student nurses were given a standard intelligence test and the Colgate Mental Hygiene Tests,—Schedule C2 for introversion-extroversion, and Schedule B2 for psychoneurotic traits.

PSYCHONEUROTIC TRAITS

The nurses revealed far fewer signs of unhealthy emotional outlets than the college girls, being more stable than 77 per cent of the arts students. One of the nurses has more psycho-

neurotic traits than most college girls, but her scholarship is poor, as the chart shows.

Those who want to specialize in children's nursing are both more introvert and more psychoneurotic than the average nurse, but are at the average of the group in intelligence. Those planning to enter industrial or general nursing are at the average of the group in all three tests.



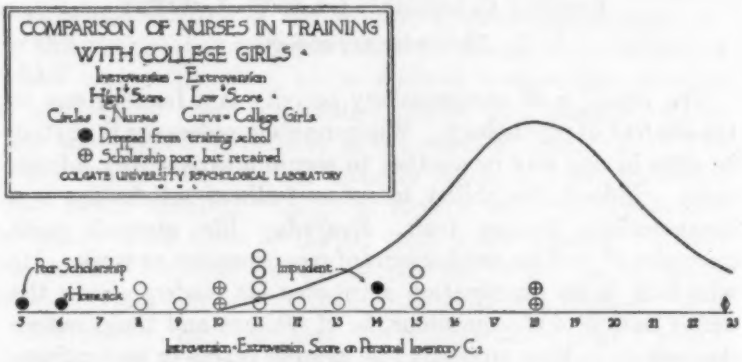
(The data for this report were scored and tabulated with the assistance of Howard Boorn, Harl C. Wolever, and Everett Schultheis.)

Is nursing an extrovert calling? The girls who enter this profession are outstandingly extrovert. The average nurse of the group tested was more extrovert than 94 per cent of all women entering colleges. The most introvert nurse in the group was only as introvert as the average college girl.

The student nurse's progress, however, is measured in the early part of her training course by her proficiency in book work and lectures. We know from other data gathered in the Colgate laboratory that extroverts are comparatively poor students. In the case of the nurses, we find a zero correlation between classroom grades and introversion-extroversion scores. The two most extrovert girls have been dropped from the

training school. One of these was the lowest of the class in intelligence.

Four of the student nurses have poor scholarship, but are being retained for their effort. All four are below the average of the group in intelligence, and, significantly, are above the average of the group in introversion.


COMPARISON OF NURSES IN TRAINING WITH COLLEGE GIRLS
Introversion - Extroversion
High Score Low Score
Circles = Nurses Curve = College Girls
Dropped from training school
Scholarship poor, but retained
COLGATE UNIVERSITY PSYCHOLOGICAL LABORATORY
Poor Scholarship
Hossack
Impudent
3 4 7 8 9 10 11 12 13 14 15 16 17 18 19 20 21 22 23
Introversion-Extroversion Score on Personal Inventory C1

Definite personality traits in which the nurses are more extrovert than college girls are:

Nurses remember better the details of things to be done.
Nurses move more quickly.
Nurses prefer working with others.
Nurses are more ready to share things, even at personal sacrifice.
Nurses are less radical in their views on political and religious questions.
Nurses give less attention to personal appearance.
Nurses are less inclined to day dream.
Nurses are not as self-conscious.
Nurses are less reserved about making acquaintances.
Nurses are less moody.
Nurses blush less.

THE COMPENSATORY FUNCTION OF THE SUNDAY "FUNNY" PAPER

HARVEY C. LEHMAN AND PAUL A. WITTY
The University of Kansas

The principle of compensatory activity is a familiar one to the student of psychology. When man's impulses are thwarted, he tries in one way or another to secure a satisfactory adjustment. Indeed, the ability to secure indirect satisfaction is a characteristic human trait. Everyday life abounds with examples of various mechanisms of compensation at work. He who fails in an examination attributes his inadequacy to the unfair nature of the questions, he of reticent and timid nature despises more than anything else aggressiveness or pretentiousness, and he who is physically weak lives with the hero of screen or stage the aggressive life of a robust man. Through projection, one secures satisfaction by attributing his own deficiencies to others, by rationalization one is able to maintain his self-esteem in the face of irrational behavior, identification permits one to substitute for genuine action imaginary achievements by identifying one's self with the conquering or wounded hero. Numerous other types of compensatory adjustment evidence the capacity of the mind to seek out tenaciously satisfying substitute activities.

Individual differences in the ability to withstand the thwarting of wants are great. So too individuals vary tremendously in the types of adjustment affected when thwarting occurs. Some individuals maintain mental calm and poise in the face of the most harassing circumstances and seldom seek satisfaction through substitute gratification; others, however, are actuated by slight deprivations to seek numerous and varied forms of substitute gratifications.

The fact of individual differences in reference to the ability to withstand thwarting is at once apparent. The present writers have observed certain racial differences in frequency of participation in certain play activities which appear to be compensatory in nature. In a recent article appearing in the *Psychological Review*, the writers displayed data showing that certain Negro children "played school" much more frequently than white children of corresponding chronological ages (1). In this article they suggested that the Negro children played school more often than white children because this activity symbolized to them knowledge, power, and prestige, which they were unable to achieve in the world of actuality.

In another investigation recently made, the writers found that Negro children engage in church and Sunday School activities much more commonly than white children. It was concluded that the church affords the Negro many *easily accessible* and intensely satisfying substitute activities.

The reader may feel that the writers have placed undue emphasis upon the concept of "escape." However, it might be said that most human activity is actuated by attempts to escape from unpleasant circumstances. In a recent volume the thesis is presented that the effort to maintain "complacency" characterizes most human behavior (2). In the earlier writings of Freud much emphasis was placed upon what he terms the "pleasure principle" (3). Freud maintained that most human behavior existed in the interest of this desire and that numerous activities not obviously pleasureable actually were symbols through which gratification of the pleasure principle was secured. Thus Freud maintained that the night-mare exists in reality usually as a symbolic wish-fulfillment. In a more recent article Freud suggests that there may be an additional principle underlying human conduct, which goes beyond the pleasure principle (4).

Certainly it is clear that most human activity is actuated by the desire to secure pleasure. The human organism is so equipped that multitudinous responses are possible to every stimulus. The organism seeks constantly satisfying states of

affairs, and tries out various modes of response until satisfyingness is attained. All life becomes then a struggle for the attainment of wants and all men manifest this struggle. But the complexity of life today constantly thwarts these wants and many modes of substitute gratification ensue. Certain activities, products of modern invention and ingenuity, afford opportunity for vicarious satisfaction of certain desires which are denied in the world of actuality.

In a series of investigations, the writers have observed conspicuous and consistent interests of children in various leisure time activities. The unanimity of interest in certain activities was characteristic of large groups of children of *all* ages. In a number of extensive investigations of the play of unselected children, the frequency with which children look at the Sunday "funny" paper was noted. Indeed this activity was more generally engaged in than any other of a list of 200 games, sports, and other play activities. "Looking at the Sunday 'funny' paper" was as commonly participated in by one sex as by the other; it was the one activity most frequently engaged in by children during *all* of the seasons of the year, and Negro and white children alike manifested an inordinate interest in it.

As far as the writers are aware, the extent to which children participate in this activity has never been quantitatively stated. The following paper reports (1) the extent to which certain groups of children took part in "Looking at the Sunday 'funny' paper," and (2) an attempt to account for the frequency of participation in this activity.

METHOD

Over 5000 children were asked to check from a comprehensive and catholic list of play activities *only those* in which they had voluntarily engaged during the preceding week. The directions were specific, the teachers were instructed in giving the tests, and the data were obtained from unselected pupils in order that the results of the investigation might be reliable. Data were secured from children in the following Kansas towns: Kansas City, Lawrence, Bonner Springs, and Moran.

In order that seasonal differences might be taken into account the list was checked by the above groups on each of three different dates, November 7, 1923, February 20, 1924 and April 30, 1924. The same list of activities was checked on each of these dates and the same procedure in administering the test was used at each testing.

In order to discover such differences as might exist between rural children and city children the list was similarly checked in certain one-teacher, rural schools, each having an average attendance of less than twenty-five pupils. These rural schools were located in Shawnee, Douglas, and Franklin counties (Kansas). Owing to the difficulties involved in administering the list to these isolated pupils, test data were secured from the rural children on two occasions only. The rural pupils checked the list in the fall of 1924. To verify the initial findings, the list was checked again in the fall of 1925. Additional data were secured from 6000 pupils of Kansas City, Missouri, in January, 1926, and the reliability of the earlier findings further verified. The total number of pupils of ages $8\frac{1}{2}$ to $15\frac{1}{2}$ inclusive from whom data were obtained is indicated in table 1.

One of the items included in the list was No. 62, "Looking at the Sunday 'funny' paper." The pupils were directed to mark this activity if they had engaged in it "just for fun" during the preceding week. From responses to this item, the writers hoped to secure significant data in regard to the *extent to which pupils now* engage in this particular activity.

Table 2 displays the percentages of children included in the various investigations who stated that they had engaged in activity No. 62, "Looking at the Sunday 'funny' paper." The data are arranged by age and by sex. It will be noted from table 2 that the data obtained in the Kansas City, Missouri, investigations were assembled separately for white children and for Negroes. This was done in order that a study of race differences might be made.

It is of interest to note that marked seasonal variation is not revealed by these data. This fact is shown best by the

data obtained from the investigations of November, 1923, February, 1924, and April, 1924, these three investigations having been made among identical groups of children. The similarity of the results obtained in each of the three seasonal

TABLE 1

Number of pupils included in a series of investigations of children's play behavior

	AGES							
	8½	9½	10½	11½	12½	13½	14½	15½
Boys								
Town boys, November, 1923	98	169	182	187	249	280	274	230
Town boys, February, 1924	90	161	169	167	201	231	252	247
Town boys, April, 1924	80	144	160	184	176	259	238	247
Country boys, November, 1924	89	102	102	106	101	93	67	46
Country boys, November, 1925	35	45	67	65	79	70	50	22
*White town boys, January, 1926	100	100	100	100	100	100	100	100
Negro town boys, January, 1926	39	75	94	89	104	80	48	40
Girls								
Town girls, November, 1923	100	174	215	235	326	269	301	261
Town girls, February, 1924	97	139	199	222	289	235	282	244
Town girls, April, 1924	99	144	176	220	266	263	278	256
Country girls, November, 1924	85	85	103	109	83	89	68	27
Country girls, November, 1925	39	49	53	64	69	57	35	21
*White town girls, January, 1926	100	100	100	100	100	100	100	100
Negro town girls, January, 1926	48	90	123	114	110	91	71	34

* A much larger number of white children were included in the investigation that was made in January, 1926. It was thought that for the purpose at hand one hundred cases would afford a fair sampling of the returns.

investigations is striking. Sex differences, too, are relatively slight, no significant sex difference being found for children of these ages in any of the investigations. Nor is any conspicuous race difference apparent in reference to participation in this activity.

Although seasonal, sex, and race differences were not conspicuous features of the data, a significant difference appeared between the town and country children in frequency of participation in this activity. This difference is brought out clearly in table 3, which shows the rank of activity No. 62, "Looking at the Sunday 'funny' paper," among the 200 items

TABLE 2

Percentages of children who reported that they had looked at the Sunday 'funny' paper during the course of one week

	AGES							
	8½	9½	10½	11½	12½	13½	14½	15½
Boys								
Town boys, November, 1923	86	90	91	87	87	88	84	78
Town boys, February, 1924	91	90	92	95	91	91	92	84
Town boys, April, 1924	84	89	86	90	88	87	87	81
Country boys, November, 1924	55	57	56	65	79	70	70	67
Country boys, November, 1925	63	69	79	82	77	69	72	77
White city boys, January, 1926	93	94	95	97	100	98	94	100
Negro city boys, January, 1926	95	90	94	94	93	86	94	85
Girls								
Town girls, November, 1923	85	89	91	83	91	89	90	85
Town girls, February, 1924	95	94	94	95	95	94	90	87
Town girls, April, 1924	85	90	95	95	94	90	87	88
Country girls, November, 1924	65	74	80	82	71	66	76	44
Country girls, November, 1925	84	65	72	70	75	77	74	81
White city girls, January, 1926	99	98	100	97	88	97	96	95
Negro city girls, January, 1926	94	100	96	92	99	97	96	97

of the list. The ranks are assigned on the basis of frequency of participation for the several groups of children of various ages. Table 3 is to be read as follows: "Looking at the Sunday 'funny' paper," is the one activity of the entire list of 200 that was found to be most commonly participated in by boys of ages 8½ to 15½ inclusive in the first three investigations. In the study of rural children made in November, 1925, this

activity ranked first in frequency for boys of age 10½, second in frequency for boys of age 11½, and third in frequency for boys of ages 8½, 9½, 12½ and 15½. The much lower ranking found in two or three instances for the rural children may be due in part to the small number of rural children included in the investigations. Nevertheless, the rankings in table 3 make

TABLE 3

How activity No. 62, "Looking at the Sunday 'funny' paper," ranked among a list of 200 games, sports, and other play activities in frequency of participation

	AGES							
	8½	9½	10½	11½	12½	13½	14½	15½
Boys								
Town boys, November, 1923	1	1	1	1	1	1	1	1
Town boys, February, 1924	1	1	1	1	1	1	1	1
Town boys, April, 1924	1	1	1	1	1	1	1	1
Country boys, November, 1924	6	4	13	8	2	6	5	5
Country boys, November, 1925	3	3	1	2	3	8	4	3
White city boys, January, 1926	1	1	1	1	1	1	1	1
Negro city boys, January, 1926	1	1	1	1	1	1	1	1
Girls								
Town girls, November, 1923	1	1	1	1	1	1	1	2
Town girls, February, 1924	1	1	1	1	1	1	1	1
Town girls, April, 1924	1	1	1	1	1	1	1	1
Country girls, November, 1924	2	2	3	2	5	6	5	11
Country girls, November, 1925	1	6	6	7	8	10	3	3
White city girls, January, 1926	1	1	1	1	1	1	1	1
Nego city girls, January, 1926	1	1	1	1	1	1	1	1

it evident at once that "Looking at the Sunday 'funny' paper," is less frequently participated in by rural than by town children. The fact that fewer rural children than town children engaged in this activity may be due to relative inaccessibility of the comic sheet. However, *even with country children,* looking at the funny paper *is conspicuously popular.*

One logically asks why it is that children look at the "funny" paper so generally. It might be argued that the popularity of the Sunday comic sheet is due to its accessibility, practically every home subscribing to one or more Sunday newspapers. This explanation, however, is inadequate. It has been shown previously by one of the present writers that accessibility alone is insufficient explanation of the popularity of certain plays and games (5).

The popularity of the comic strip cannot be attributed to its artistic appeal or to the accuracy with which it presents *actual* life situations. The comic sheet is an artistic affront and life situations are distorted almost beyond recognition. The colors usually are presented with no regard for harmony of combination, serving merely as means for quick and unmistakable identification of character.

The lines giving form to the characters serve an additional purpose. They *suggest* certain human characteristics in exaggerated or distorted form. One character through typical and unvaried form comes to be associated invariably and unmistakably with a particular human trait or combination of traits. Each character exemplifies certain commonly encountered human frailties or vanities. These are naively and crudely presented, but so directly and unmistakably that even the most obtuse cannot fail to recognize them.

The action of the funny paper centers about certain characters in whom *restraint* and *suppression* are conspicuously absent. The character of the Sunday 'funny' section takes many risks and defies, frequently with complete immunity, both natural and man-made laws. In some instances he is pictured as succeeding in his endeavors by recourse to easy magic. At other times he is pictured as falling squarely upon the face or being severely burned with fire or scalding water. He falls a great distance or he is closely pursued by dangerous wild animals. He assaults his fellow man with murderous vengeance and is in turn assaulted. He knows no decorum. He puts his cigar ashes into an upturned silk hat, or into the gold-fish bowl. He rests his feet on his neighbor's lap. He

speaks with saucy abandon to dignified personages. He endures no suppressions. He has complete freedom in self-expression.

Too he has small regard for property rights or for the preservation of property. He handles valuable articles with reckless unconcern and he destroys large amounts of wealth in various forms.

This lack of self restraint is demonstrated further in the use of language which is usually incorrect and indecorous. Such mutilation of English form is seldom encountered in actual life. Such usage permits the character unrestrained and uninhibited action in verbal situations.

In this somewhat brief analysis of the comic paper, the writers have emphasized the unimpeded activity of the characters, the complete lack of restraint, and the extreme freedom of expression. The writers feel that the popularity of the comic strip is due largely to the fact that it presents *unhampered* human activity through which the reader vicariously satisfies his *thwarted* and *restrained desires.*

The writers suggest that the pleasure that children find so universally in the Sunday "funny" paper is based also upon such motives as escape, mastery, etc. Woodworth has explained how the escape motive operates.

> The "escape motive" deserves a little more notice. Though you would say at first thought that no one could seek fear, and that this instinct could not possibly be utilized in play, yet a great many amusements are based on fear. The "chutes," "scenic railways," "roller coasters," etc., of the amusement parks would have no attraction if they had no thrill; and the thrill means fear. You get some of the thrill of danger, though you know that the danger is not very real. Probably the thrill itself would not be worth much, but being quickly followed by *escape*, it is highly satisfactory. The joy of escape more than pays for the momentary unpleasantness of fear. . . . . Nothing could be much further from the truth than to consider fear as a purely negative thing, having no positive contribution to make in human satisfaction. Though we try to arrange the serious affairs of life so as to avoid danger as much as possible, in play we seek such dangers as we can escape by skilful work. The fascination of gambling and of taking various risks probably comes from the satisfaction of the fear and escape motive" (6).

The child who looks at the Sunday "funnies" is enabled to identify himself with the most intrepid adventurer, or the most resolute law-defying criminal, or the capturer of such a criminal. On the other hand he may identify himself with the serial-motion-picture type of hero who wins out over apparently insuperable odds. Thus the Sunday "funny" paper provides the child with a means of shooting the chutes; it enables him to get all the thrill of danger with escape secure. In the world of actuality the child is obliged to conform to certain conventions, to treat other persons with a reasonable amount of decorum, and to abide by the consequences of his acts when he defies natural law or human authority. In the "funnies" he may defy every law and he may do so with immunity.

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PREDICTING ABILITIES FROM INTERESTS[1]

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The statement by an individual that he is interested in a certain job, a course of training, a particular career, is regarded as significant evidence of ability by many who are concerned with the educational and industrial problems of personnel selection and guidance. Educators have rightly said that an interest is necessary to pleasurable effort and through interest success becomes more probable. This has been interpreted to mean that the verbal expressions of interest are indicative of abilities.

The theory that interests are predictive of abilities has been widely used in building up the practical procedures of both the employment office and the guidance clinic, although some workers were early concerned with how much weight should be given to these interest expressions. The problem was formulated for investigation in industrial psychology some fifteen years ago. Thorndike (13, 14) gave to the view that interest expressions are indicative of abilities a scientific authority by establishing a relationship of +0.89 between educational interests and self-estimated abilities. Practically, however, the theory fell down under the test of personnel workers. Kitson (9, 10) voiced the experience of personnel executives and vocational counselors that interests are only slightly suggestive of abilities. To know whether the ordinary statements of interest have genuine significance and to know exactly how significant these verbal expressions are in predicting abilities in the fields of industry and education has been the purpose of the investigations summarized here.

[1] Paper read before the thirty-fifth Annual Meeting of the American Psychological Association, December 28, 1926.

THE INVESTIGATION OF THE PROBLEM

The investigation of the relation of vocational interests and abilities has been carried on among two samples (5, 6): one, a mixed group of 65 students in a western college, and the second a group of 67 women in an eastern college. To make the test among individuals in the occupations an investigation has been carried out among 320 young men already employed, but applying at a bureau in New York City for vocational guidance (4). In addition, a limited sample of 17 women in an advanced vocational school has been studied to check upon differences in this field (5).

To measure the relation between the vocational interests or ambitions and abilities, the vocational ambitions were first rated according to the average intelligence of workers already in these occupations (3), after which the two variables: (*a*) intelligence of subject and (*b*) intelligence requirement of vocational ambition, were compared by percentage and correlation methods. For the method of percentage an arbitrary assignment of a probable errror of one and one-half years mental age was made. If the intelligence of the subject and the intelligence requirement of the vocational ambition were within a range of three years mental age there was considered to be perfect resemblance, or vocational adjustment for the subject if he were placed in his vocational ambition.

RESULTS BY METHOD OF PERCENTAGES

For the college subjects there is fairly close agreement in the three periods for which data were secured, namely, elementary school, high school and college. The percentage of those within the probable error range of three years mental age, and thus approximating an ability adjustment in their vocational interest, is represented by six measures ranging from 11 to 50 per cent. The per cent of those approximating a correct choice is higher for each of the three periods among the eastern sample of college girls than it is among the western mixed sample.

It is interesting to note a general increase in vocational adjust-

ment in both groups, paralleling advancement in education. For the two groups this runs as shown in table 1.

For the above two groups the data secured for the elementary school and high school periods were given in retrospect. Also, it can be considered as only representative of those individuals who go to college. Verification of the results for these two periods is necessary. Data from 806 high school boys and girls, offered in a study of vocational ambitions by Proctor (12) have been used to check upon the relation for high school students. An investigation into this relationship among 512 elementary school graduates upon entering high school has been made by Feingold (2). Fifty-nine per cent of Proctor's high school subjects have the same intelligence as that required by their vocational ambition and 46 per cent of Feingold's subjects.

TABLE 1

Per cent approximating correct vocational choice

	ELEMENTARY SCHOOL	HIGH SCHOOL	COLLEGE
Western group (mixed)	11	22	38
Eastern group (girls)	39	32	50

These figures for those approximating vocational adjustment are higher than figures for the elementary and high school periods offered by the two college groups.

These results suggest for average elementary school pupils (later years) that about 40 per cent will approximate their ability in their vocational interest or make a correct vocational choice. This is an interpretation taking into consideration the several group influences upon the results. Individuals of superior intelligence in elementary school, who go on to college, appear to make in their interest expressions a poorer vocational adjustment than those of average intelligence. The average high school pupil makes a better vocational choice than does either the elementary or college subject. The several results from high school pupils suggest that, while the choice of those

who go on to college are less likely to be correct, the average high school pupils (other than at graduation) can be expected to approximate adjustment in their vocational choice to the extent of about 50 per cent. For college, with results from two groups of 38 per cent and 50 per cent, it can be expected that about 45 per cent will approximate a correct choice. College students are of an intelligence not required by most of the occupations of society. A chance selection of an occupation would be less likely to place the college student in an occupation suited to his general qualifications than it would the high school student, and even much less likely than the elementary school graduate, because of the preponderance of occupations requiring average, rather than superior, intelligence. This

TABLE 2

Vocational adjustment of ambitions of vocational school pupils

PERIODS	PER CENT WITH APPROXIMATELY SAME INTELLIGENCE AS REQUIRED BY VOCATIONAL AMBITION
Elementary School	67
High School	53
Vocational School	74

would seem to be the influence causing the lower percentage for college students.

For those in vocational school, requiring high school graduation for admittance, there are figures for the three periods (table 2). Of these advanced vocational school students, all of them girls training for domestic science, the percentage of vocational adjustment is much higher than for any of the other groups; only about 25 per cent would be maladjusted according to ability if placed in the vocation of their interest at the time this investigation was made. It would seem that these individuals already had been selected on the basis of ability. College has dropped them or refused them because of their inability to carry

collegiate work, and they naturally gravitate into training for which they have ability and in which they soon gain an interest. In 93 per cent of the cases the vocational ambition of these subjects was the one for which they were in training.

Among those individuals in the occupations, but seeking further vocational opportunities, where occupational knowledge is undoubtedly greatest, it would seem that the vocational ambition could be used with some prognostic authority. These individuals are evidently less ignorant of the requirements of various occupations than are the members of the other groups mentioned. However, 44 per cent of the occupational group investigated were of the same intelligence as that required by

TABLE 3

Vocational adjustment of ambitions at different educational periods

PERIODS	PER CENT APPROXIMATING INTELLIGENCE REQUIRED BY VOCATIONAL CHOICE
Elementary School (later years)	40
High School (other than at graduation)	50
College (other than at graduation)	45
Vocational School (requiring high school graduation)	75
In Occupation (between ages of twenty and thirty)	45
All periods	50

their vocational ambition. For those in the occupations, between the ages of twenty and thirty years, it is probable that not more than 45 per cent approximate on an ability basis a correct vocational choice by their vocational ambition.

SUMMARY OF RESULTS BY METHODS OF PERCENTAGES

When all the results reported are assembled, 1816 cases in total, there are 49 per cent of the individuals whose intelligence is within a three-year-mental-age range of the intelligence requirement of their vocational ambition. These are considered to approximate a correct vocational choice. An interpretative summary of the results for the separate periods of activity for which data were secured is given in table 3. These figures may

be viewed as chances in one hundred of approximating correct vocational choice. That is to say, there are, generally speaking, about fifty chances in one hundred of approximating a correct vocational choice. This is, of course, the same chance one would have in tossing a coin.

These results have been derived by the method of percentage. The results which follow, derived by a more exact method, that of correlations, are very similar.

THE CORRELATION COEFFICIENTS AND THEIR SIGNIFICANCE

The probable relation for elementary school students, as expressed by the correlation coefficient, between the two variables, (*a*) the subjects' intelligence, and (*b*) the average intelligence requirement of the occupational ambition, is represented by two correlations of -0.17 for the eastern group of college girls and $+0.22$ for the western mixed sample. As the data upon which these results are based were secured from college students and in retrospect their representation of ordinary elementary students may be questioned. Feingold's (2) investigation of elementary school graduates gives the correlation for boys as $+0.05$ and for girls $+0.10$.

It is evident that the usual relationship for elementary school subjects is not higher than $+0.10$.

For high school students the relation is expressed by two correlation coefficients of -0.20 for the eastern group and $+0.08$ for the western group. The data upon which these results are based are subject to the same criticism in so far as considered representative of ordinary high school students as that for the elementary school period. A correlation worked from Proctor's (12) data gives this relation as $+0.21$, which makes it evident that the usual relation for high school students is seldom better than $+0.20$.

For college students there are the two correlations of -0.22 for the eastern group and $+0.06$ for the western group. Using school grades as the criteria of ability, instead of intelligence of the students, a coefficient of -0.11 is found for the eastern group. It is evident that the usual relation between voca-

tional interests and abilities for college students is not higher than +0.10.

For those students in advanced vocational school there is a correlation coefficient of +0.58 between intelligence and intelligence requirement of the vocational ambition and for those in the occupations there is a correlation of +0.38.

In summary, it would appear that the usual relationship between (*a*) the subjects' intelligence, and (*b*) the average intelligence requirement of the occupational interests, is not higher than that expressed by the following correlation coefficients:

For elementary school (later years)	+0.10
For high school (other than at graduation)	+0.20
For college (other than at graduation)	+0.10
For advanced vocational school	+0.60
For occupational work (ages twenty to thirty)	+0.40

None of these correlations are high and, excepting the groups of individuals in the occupations and in the advanced vocational school, they are exceedingly low. The coefficients of alienation for these correlations run from 0.98 and 0.99 for students in elementary school, high school and college to 0.91 for those in the occupations and 0.80 for those in vocational school. These coefficients indicate the per cent of error in estimating ability from interest with zero correlation representing 100 per cent of error. In other words, the probable error of estimation has been reduced by 1 per cent for elementary school subjects, 2 per cent for high school subjects, 1 per cent for college subjects, 20 per cent for students of advanced vocational school, and 9 per cent for those in the occupations.

The prediction of ability by vocational interests may be interpreted in terms of chances of correct estimation.[2] At best there are sixty chances in one hundred. This is the situation for vocational school subjects. For those in the occupations there

[2] For statistical explanation of this interpretation of the correlation coefficient in terms of chances in one hundred the reader is referred to paper by Douglas Fryer and Lyle H. Lanier, "Prediction in Terms of Chances in 100 from the Correlation Coefficient," *Industrial Psychology Monthly*, 1927, ii, pp. 254–263.

are fifty-four chances in one hundred; for those in elementary school, high school and college there are between fifty and fifty-one chances in one hundred for the estimation of vocational abilities from vocational interests. The slight difference between these predictive values and those found by the method of percentages have their explanation in the method and depend upon the probable error used.

IMPORTANCE OF INTEREST EXPRESSIONS AT DIFFERENT MENTAL LEVELS

A secondary problem may be stated as a question. Can we expect individuals of the higher grades of intelligence to make

TABLE 4

Relation between intelligence and requirements of vocational ambitions in various intelligence groups

INTELLIGENCE GROUPS IN MENTAL AGE	NUMBER OF CASES	PER CENT MAKING CORRECT VOCATIONAL CHOICE ON BASIS OF INTELLIGENCE
18 and above	333	39
17 to 17–11	269	63
16 to 16–11	310	72
15 to 15–11	302	52
14 to 14–11	226	54
13 to 13–11	149	11
12 to 12–11	49	10
9 to 11–11	10	0
All intelligence groups. . . .	1,816	49

vocational choices more closely approximating their intellectual ability than individuals of lower grades? To answer this question a large number of the cases from the investigations previously presented, a total of 1816, have been assembled into mental age groups. In this mental age distribution there is found a progressive increase commencing at the ninth year with zero per cent approximating a correct vocational choice up to the sixteenth year, with 72 per cent. At the seventeenth year there is a decline, with 63 per cent approximating a correct vocational choice and at the eighteenth year, 39 per cent.

The percentage of those whose intelligence approximates that required by the vocational ambition markedly increases from the lowest grades up to the seventeenth year. For the seventeenth year and years following there is a decline. The explanation of this would seem to lie in the difficulty that high grade ability has in finding satisfactory outlet in the occupations, the same difficulty that confronted the college student.

With the exception of the decrease at these upper intelligence levels, *higher intellectual ability* appears to be correlated with *correct vocational choices*. The chances that an individual above eighteen years mental age will make a correct choice is 39 in 100; the chances that an individual of sixteen years mental age will make a correct choice is 72 in 100. For the individual of fourteen years mental age it is 54 in 100 and for the individual of twelve years mental age there are 10 chances in 100. The lower an individual is in degrees of mentality the greater he is likely to be in error in vocational interests.

THE RELATION OF EDUCATIONAL INTERESTS AND EDUCATIONAL ABILITIES

A full discussion of the relation of educational interests and educational abilities will be found elsewhere (7). Investigations of the problem have been carried on among two groups of college students, a mixed sample of 104 students in a western college and the other a group of 89 men in an eastern college. Measures of relationship have been made for each subject between rank in educational interest and rank in ability as estimated by the student, also, between rank in interest and rank in educational subject according to school grades. Data were secured for three different periods, namely, elementary school, high school and college. The results for these three periods are in close agreement, as are the results from the two groups under study. The probable relationship between educational interests and educational abilities according to school grades is represented by six median correlations ranging from $+0.04$ to $+0.57$.

Bridges and Dollinger (1) have reported results upon this same problem, offering data reworked by Thorndike (15). Between rank in educational interests and rank in college grades a median correlation of +0.46 was found.

From all the results it would appear that the usual or probable correlation between educational interests and educational abilities according to school grades is about +0.30. This correlation gives a coefficient of alienation of 0.95. In the prediction of abilities by the educational interests this may be interpreted to mean that there is a 5 per cent decrease in the error of estimate, which offers fifty-two chances in one hundred of correct prophecy.

The relationship between educational interests and estimated abilities (by the individual) is represented by eight median correlations ranging from +0.50 to +0.70 for which the most usual relationship is about +0.60. Data of Bridges and Dollinger (1) give a correlation for estimated abilities of +0.70, which is equal to the highest found in the writer's investigations. Accepting the usual correlation between interests and estimates of educational ability to be about +0.60 the coefficient of alienation is 0.80. This offers sixty chances in one hundred for the prediction of abilities by educational interest expressions.

OTHER APPROACHES TO THE PROBLEM

There have been other approaches to the problem. Poull (11) studied the occupational interests of 1206 elementary school children from a working man's neighborhood of mechanics and skilled trades in New York City. When these children, who inherited similar ability and would have the ability required by similar occupations to their parents, chose vocational interests in these skilled trades and mechanics, they approximated in their intelligence the requirements of the occupation. But when they chose clerical or professional work, as about 50 per cent of them did, it was found that the distribution of the children's intelligence ratings was almost 25 per cent lower than the distribution of the occupational requirements for clerical work and almost 50 per cent lower than for the professions.

The measures of the ability of 31 college students in six tests of intelligence and their ranking of these six tests in interest upon completing the six different tasks have been compared in an investigation by Hartman and Dashiell (8). An average correlation of +0.40 was found to exist between these two variables.

RELATIVE VALUE OF VOCATIONAL INTERESTS AND ACTUAL OCCUPATIONS IN PREDICTION OF ABILITIES

A practical test of vocational interests as criteria for the prediction of abilities is found in a comparison of the predictive value of the vocational interests with the predictive value of the actual occupations in which the individuals expressing these interests are at work. The range of three years mental age, within which vocational adjustment is considered to be perfect, was used here as in previous studies of vocational interest. Fifty-one per cent of a group of 320 individuals applying for vocational counsel at a bureau in Brooklyn would be maladjusted if placed in their occupational ambition. On the other hand, if left in the actual occupation, 60 per cent would be maladjusted on the basis of their ability.

But when the two criteria for vocational selection and guidance, (*a*) the actual occupation and (*b*) the desired occupation are correlated with intelligence, it is found that the actual occupation shows a slightly better vocational adjustment. The correlation between the two variables, intelligence of subjects and intelligence requirement of actual occupations is +0.42 while between intelligence of subjects and intelligence requirement of the desired occupations it is +0.38.

In answer to the question: Which is the more significant as a criterion for vocational selection or guidance in placing the individual in work adjusted to his intelligence, the desired occupation or the actual occupation, we must say that no great difference appears from the standpoint of prediction. The actual and the desired occupations are about equal in prognostic significance. Following either, vocational maladjustment of about 50 per cent may be expected.

SUMMARY

The facts are quite clear regarding the problem of the relation of interests and abilities. Interest expressions—vocational and educational ambitions—are of no significant value as criteria of abilities. This was found to be true both in the study of vocational and of educational interests.

Considering in general the predictive value of interest expressions for educational and vocational selection or guidance we are faced with the facts which indicate that in the actual

TABLE 5

Probability table for the prediction of vocational abilities from vocational interests at different training or developmental periods

PERIODS	NUMBER OF CHANCES IN 100 FOR THE PREDICTION OF ABILITY FROM VOCATIONAL INTERESTS	
	From method of percentages	From method of correlations
Elementary School (later years)	40	About 50
High School (other than at graduation)	50	About 50
College (other than at graduation)	45	About 50
Vocational School (requiring High School graduation)	75	60
In occupations (between 20 and 30 years of age	45	54

In this table the results from the method of correlations is considered the more exact. The predictive value at the different periods suggests the influence of age and knowledge of the occupational world upon the relation of vocational interests to ability qualifications.

use of these interests there is little more than suggestive value. In the guidance of a youth into a vocation or into an educational course his interest in the vocation or course of education is not at all indicative of ability in this vocation or course. It is only slightly suggestive of such ability. The same situation is true in the selection of workers in industry or in students applying for educational courses, college majors and minors, and so on. Interest may be allied with ability in particular cases, but, on the average, it cannot be relied upon for predictive purposes.

The lack of practical predictive value of vocational interests is well illustrated in a comparison of the actual and desired occupations of individuals seeking vocational guidance. The occupation desired, the vocational interest, is found to have only about equal predictive value of vocational ability to the

TABLE 6

Probability table for the prediction of vocational abilities by vocational interests at different intelligence levels

INTELLIGENCE LEVEL IN TERMS OF MENTAL AGE	NUMBER OF CHANCES IN 100 FOR PREDICTION OF ABILITY BY VOCATIONAL INTERESTS
All ages	49
9–11	0
12	10
13	11
14	54
15	52
16	72
17	63
18 and above	39

This table is based upon results by the method of percentages.

TABLE 7

Probability table for the prediction of educational abilities from educational interests

METHODS USED	NUMBER OF CHANCES IN 100 FOR PREDICTING EDUCATIONAL ABILITIES BY EDUCATIONAL INTERESTS
From use of estimates of ability	60
From use of school grades	52

This table is based upon results by the method of correlations.

actual occupation in which the individual is at work. An occupation in which an individual would express his ability to greatest advantage could, on the average, be predicted equally as well from the occupation in which he is engaged as from the occupation toward which he aspires.

The specific predictive value of the verbal statements of interest is shown in the probability tables (tables 5, 6 and 7), which have been prepared upon the summarized results of investigations given above.

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AN EXPERIMENT ON THE LEARNING OF "PAIRED ASSOCIATES"

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This paper describes a simple experiment of the "learning" type which points to the comparatively short time and small amount of practice required for undergraduate students in the laboratory to duplicate one of the exhibitional feats of a "memory expert;" also it presents some figures on the predictability of "learning" from test scores, together with some theoretical questions raised regarding certain general problems in the field of "learning."

Four years ago the writer was a guest at a luncheon club in a Michigan city. At this meeting a widely advertised "memory expert" entertained with various exhibitions of his "powers," preliminary to the usual business of organizing paying classes, private lessons, etc. Most of the spectators were amazed at the demonstrations in much the same way that elementary students are frequently impressed when first introduced to the psychological laboratories and their activities. One of the "feats" consisted in retiring into a small room with a newly shuffled deck of playing cards for twenty minutes, after which the man emerged, was blindfolded and then proceeded to call out each of the 52 cards in order without error. The procedure was then repeated after ten minutes, as a check, with the same errorless result.

At that time the writer raised the questions: How long will it take ordinary undergraduates to duplicate this "feat" in the laboratory? How well can the rate of learning the 52 "paired associates" be predicted from the scores made by freshmen on "intelligence" tests of the Alpha type?

Fifty students[1] in the elementary laboratory course were set to this problem. They were freshmen, except for a sprinkling of sophomores: thirty-five women and fifteen men.

The literature abounds in studies on the effects of practice on the "range of attention" and of "visual apprehension."[2] In 1859 the famous "conjurer," Robert Houdin,[3] with his son, Emile, tried the estimation of the number of dots on dominoes until, he says, "We were at length able to give instantaneously the sum of a dozen dominoes." Also, "My son and I passed rapidly before a toy shop window . . . . and after casting an attentive glance, a few steps further on . . . . he (Emile) could often write down as many as forty objects . . . . and rarely made a mistake."

In our experiment we sought to determine how long the practice must be extended to reach some such skill as that of the younger Mr. Houdin or of "Addison Sims of Seattle."

THE EXPERIMENT

The subjects, working in pairs, were given decks of playing cards and instruction sheets which pointed out that each card represented a suite and a position in that suite and that these were to be learned in the most effective way without any undue pressure or "cramming." At each sitting the deck was first thoroughly shuffled. The subject then took the deck, face-downward, in his left hand and each three seconds turned the top card face up on the table and spoke aloud its name. As soon as the last card was turned the experimenter recorded the total "impression" time and then proceeded to test the subject who turned his back and called out the cards, beginning with No. 1.

[1] From the Michigan Normal School, Kalamazoo.

[2] The Cornell laboratory alone has turned out a dozen or more papers by Whipple, W. S. Foster, Josephine Curtis Foster, Dallenbach and others.

[3] Life of Robert Houdin, the king of the conjurers, by himself. Philadelphia, 1859, Ch. XIV, p. 256. Also cited by Whipple, Manual, 1, 278–290.

These the experimenter recorded against the original order, and after pointing out the subject's errors, the "impression" series was again resumed without disturbing the original shuffled order. Thus the "impression"-testing was alternated until the subject's total "impression" time for the sitting was twenty minutes. Sittings were continued tri-weekly until the subject after twenty minutes of "impression" could call out the 52 cards twice without error. Each subject was left completely free to pursue his own method.[4] Careful protocols were insisted upon. These were jotted down while the experimenter was making the check and were valuable not only as short rest pauses from the labor of "impression" but afforded frequent opportunities for the overhauling of method. Consequently it might be said that the subjects worked "with knowledge," although the total number of right cards, inversions and insertions of order was not revealed to them until the end of the entire sitting.

RESULTS

At the conclusion of the experiment we had for each of the fifty subjects the record of his work at each sitting together with his own protocols, the score of each on the Army Alpha test taken the preceding quarter, and the marks in the elementary course in psychology of the preceding (Autumn) quarter.

Table 1 shows in double entry the number of twenty-minute "practices" required to master the deck (columns) against the Alpha scores (rows). Beginning at the top right hand corner the table reads as follows: One student learned the 52 cards the first sitting and he scored between 170 and 179 points on the Alpha. One student learned the cards in three sittings with about the same Alpha score. Another student took four sittings, with about the same Alpha score, etc.

[4] That is, free (beyond the scope of the instruction) to use any grouping, rhythm or mnemonic system he might elect. The protocols later show the wisdom of making clear to the subject *all other* details of method save this one.

CONCLUSIONS AND DISCUSSION

1. The average freshman was able to extend his "range of visual apprehension" to the equal of that of the professional "memory expert" in 5.25 sittings of twenty minutes "impression" time each. Two students did it the *first* sitting, i.e., "without any training at all." Twelve, or about one-fourth of the subjects equaled the "feat" on the third sitting, while thirty-eight (76 per cent) of the subjects had reached the ob-

TABLE 1

Distribution of the twenty-minute practices in learning the cards and the scores on the alpha test

ALPHA SCORES	PRACTICE PERIODS													TOTAL
	13	12	11	10	9	8	7	6	5	4	3	2	1	
170–179										1	1		1	3
160–169								1		1		1		3
150–159			2			1	2	1			3			9
140–149	1					1			1	1	1			5
130–139								1	1	3	1			6
120–129		1						1	1	1	1	1	1	7
110–119					1		1	1		1				4
100–109					1		1	2	3	1				8
90–99									1	2				3
80–89									1					1
70–79														
60–69										1				1
Total.....	1	1	2		2	2	4	7	8	11	8	2	2	50

jective in six sittings, i.e., in two weeks of practices of but twenty minutes each on Mondays, Wednesdays and Fridays.

2. An examination of the protocols of the four students who required most "practice" (11, 11, 12 and 13 sittings respectively) shows *invariable weakness of method* in contrast to that of the quick learners. Miss F. R. S. who had 13 sittings and was therefore one of the slowest learners reports in her protocols:

> In my first nine sittings I worked hard as I could, *trying out various mnemonic systems,* [italics mine] . . . . but my scores were, on

these nine days, only 8, 4, 7, 11, 14, 9, 20, 15 and 15 "rights." I decided on the ninth day that these systems were hindering rather than helping me, so I abandoned them and simply tried to look carefully at each card while I called out its name. On the tenth day I noted no improvement with this new method, but my later scores tell the story. For the tenth, eleventh twelfth and thirteenth days they were 21, 50, 30 and at last 52. On the tenth day the learning in some way just seemed to come all at once.

This woman was thirty four years of age, a grade school teacher.

Miss S. V. V., who had 9 sittings reported:

I have always had a dread of committing to memory. During my first five sittings I kept saying to myself "You never can do this." (Her records for these days were 10, 14, 11, 11 and 11 "Rights" respectively.) I then resolved to do or die. My sixth day's work netted 17 "rights" which encouraged me. The seventh day was also a 17. I again doubted if I would ever make it. My eighth day, much to my surprise, came much easier and I got 30. My ninth day gave me a still greater surprise, for I was able to give the required 52 twice without error after the fifth leafing through the deck."

The work of the slow learners seems to be characterized by (1) inhibitory attitudes or self-imposed instructions which subvene failure, or (2) attempts to short-cut by resorting to mnemonic devices which later were abandoned as ineffectual.

3. Practically all subjects reported what has been called the *Anlauf* or *initial torpor* as well as a rather sharp inflection point after which the "learning" seemed to come with great rapidity. Here with nonsense material we see the same rapid 'drop in the curve' which Köhler attributes to "insight" in his anthropoid experiments at Teneriffe. A few instances from the protocols will suffice:

It seemed no progress at all was made the first 3 periods. After the fifth trial I seemed to make rapid progress and then on the seventh I found almost all at once I had learned the whole deck. I made practically no improvement in my second sitting over the first, but in the third my learning *seemed to crystallize* [italics mine]—seemed to come all at once.

The record for this subject was 32, 35, 46, 52.

4. There seems to be a strikingly low correlation between the rate of learning as measured by the number of sittings[5] and the Alpha test scores.

If we assume linearity and compute on that basis from our correlation plot we find

$r_{xy} = 0.04 \pm 0.08$
$M_x = 5.25$ periods; $\sigma m_x = 0.37$ periods
$M_y = 132.5$ Alpha points; $\sigma m_y = 3.60$ Alpha points

The standard deviations of the distributions were of the order

$x = 2.65$ periods
$y = 25.51$ Alpha points

The two regression equations were computed as

$x = 0.04\, y$
$y = 0.38\, x$

While the standard errors of estimate were of the order

$S_x = 2.62$ periods
$S_y = 25.25$ Alpha points

5. There is almost no relationship between the test scores on the Alpha and the number of sittings on the "learning." Noting the orders of the two regression equations, the predictive value of the Alpha scores is practically zero, while there is some slight drift in the direction that those who "learn" in a few sittings are also the ones who exceed the average Alpha score. This can be seen from inspection of the plot of table 1.

[5] Here the word *rate* is uncritically used, i.e., as a mere ratio. Strictly *rate* can not be measured by any such simple means. To express "learning rate" would require that the acceleration be set down as the second differential coefficient of distance ("progress") with respect to time. Since the "practices" are probably *not limited solely* to the twenty-minute sittings, *something* (even deceleration) must go on in the interstices; so, it must be clear that *any* mathematical expression of "learning" to fit the facts must be even more complex than are the various "functions" in the equations of L. L. Thurstone, G. S. Snoddy or M.F. Meyer, if we take the view that "learning" represents some such thing as is represented in the recent theoretical papers of Joseph Peterson or L. K. Frank.

6. One source of error in the correlation figures is that the two samplings (test scores and learning) were not taken *simultaneously*, i.e., at least on the same day. Another is that the two intercepts of a theoretical "practice curve" of test performance and "learning" performance beginning at some zero point very early in life are probably incomparable because of the divergence in the stages of development of these two sets of habits (between utter näiveté and virtuosity) which each *now* represents. The gross effect of such procedure would be that when a correlation coefficient is calculated with such an error present the coefficient is thrown farther toward zero the greater the discrepancy.

7. When elementary psychology marks are compared with the "learning of paired associates" there is likewise little resemblance. The top fourth of the "learners" averaged C in scholarship (2.58 points on a scale of 0 to 4, corresponding to the E to A series) and the low fourth averaged C− (1.75 points). These two points just mark off the boundaries of the C or average interval of the quarter marks in elementary psychology for the 50 subjects.

8. In view of this and similar studies the writer is convinced that the fictitious plotting of "learning curves" in which uncontrolled and often unaccounted shifts in both *problem* and *method* take place from sitting to sitting brings about a state of affairs which essentially means that *the "learner" is only working at the same task in the sense that someone else has called it "the same."* There is no psychological justification for this.

Again the *form* of the "curve" is or has been chiefly an artificial function of the size of the time unit employed in plotting or recording. When we talk of "plateaus" what we mean to say is that we have not pushed the analysis into fine enough units of time or movement. When we do this there probably are no "plateaus." This whole question, of course, takes one immediately into the very heart of psychological system and theory. One thing, however, seems certain: the present method of representing "learning" as so much pro-

duction per unit of time, with purely arbitrary interpolation of the interims between "practices" can never lead us to psychologically sound facts about learning. It is probably that all the "step-wise" phenomena fall within this same scope, so far as "learning" is concerned.

The writer hazards the guess that the *Anlauf* or "initial torpor" phase is just a way of saying that during this time there is no improvement *in production* (although there may be in true learning!) because the learner as yet has no problem or is engaged in making various precurrent or exploratory attempts while he observes what happens when this or that is tried. And the point marked by the rapid inflection in the "curve" is probably his hitting upon the combination of result-getting processes, later to become fixed (rapidly or slowly) in proportion to the magnitude of the opportunities for variation in the series. *How* this comes about is the real problem of "learning." At present we have many theories but meagre facts. The sudden break probably represents what Swift, Ruger, Ordahl and others have called the "law of the unconscious adoption of method."

Shall we therefore attempt further to write equations for the learning function, or shall we make more crucial experiments, preferably for technical reasons in the realm of the comparative psychology of the invertebrates, seeking for facts about "learning" in the prime form and deriving from these studies new methods for work with human subjects?

NOTES AND NEWS

The Fourth International Congress of Psychology and Psycho-technique will be held in Paris October 10–14, 1927. Professor Ed. Claparede, Geneva, is President, and J. L. Lahy, Paris, Secretary-General.

The University of Pennsylvania announces the following promotions: Drs. Samuel W. Fernberger and Karl G. Miller to Professorships of Psychology; Henry E. Starr, Robert A. Brotemarkle, H. Sherman Oberly to Assistantships in Psychology.

Johns Hopkins announces leave of absence for 1927-1928 to Prof. Knight Dunlap, Experimental Psychology. Dr. Dunlap will spend the year in Washington as chairman of the Division of Anthropology and Psychology of the National Research Council. Dr. Karl Bühler, Professor of Philosophy and Psychology, University of Vienna, will give lectures in Child Psychology, first semester. Dr. J. E. Coover, Leland Stanford University, will lecture on Statistical Method and the Psychology of Learning.

Yale University reports the appointment of Dr. Edward S. Robinson to a Professorship in Psychology.

The Vocational Adjustment Bureau, 336 E. 19th Street, New York City, reports the following: Charlotte Pondee, A.B., Vassar, Placement Counsellor; Alice Bessey, Katherine Treat, Psychologists; Edna Unger, Research Assistant; Alta Frank, Coördinator. The Director, Emily Burr, reports the following investigations: Regional Survey of the Industries in the Bronx; Team of Tests Applied to Subnormal Girls in Machine Operating Class; Job Analysis Technique; also New Association for Study of Personality.

Brown University has set aside a building to be used exclusively as a laboratory of Experimental Psychology. Dr. Leonard Carmichael has resigned as Assistant Professor of Psychology at Princeton to become Associate Professor of Psychology at Brown and Director of the Psychological Laboratory.

Cornell University announces the completion of the first 30 volumes of *The American Journal of Psychology*. Dr. Elsie Murray will join the

teaching staff in Psychology at the University of Illinois. Dr. J. P. Guilford, formerly instructor of Psychology at Cornell and the University of Illinois, has accepted an Assistant Professorship at Kansas University. He will succeed Dr. C. Rosenow. Mr. John G. Jenkins, instructor of Psychology at Iowa State College has accepted a position in the University of Illinois. Mr. George Kreezer, Assistant, 1925–1927, has been promoted to an Instructorship.

The Survey of Indian Affairs, whose personnel was appointed by the United States government, has granted to Dr. T. R. Garth, University of Denver, financial assistance in order that he may bring to completion one of his many researches in comparative Indian intelligence, etc. The specific question raised is whether the common schools are just as efficient for the Indian children as are the more expensive United States Indian schools. Readers of this JOURNAL will await with keen interest the scientific answer to such a practical problem. Dr. Garth also announces that he has been collecting data on the relative ability of Mexican as compared with Indian and white children.

BOOK REVIEWS

WILLIAM BROWN AND GODFREY H. THOMSON. *The Essentials of Mental Measurement.* Cambridge: The University Press, 1925. 224 pp.

The first edition of *The Essentials of Mental Measurement* appeared in 1911; it was written by Dr. Brown. The second edition, produced ten years later by Dr. Thomson, was a thorough revision of the first edition in some two-thirds of its contents. Such revision as is incorporated in the 1925 edition appears in the form of footnotes and a brief appended chapter in which the authors state their present position (1924) on controversial matters.

A review of the main body of this important work would be out of place at this time. The controversy between Thomson and Spearman (and their followers in this country as well as in England) as to intelligence theories is well known by readers of this Journal. Spearman's Theory of Two Factors, supported by elaborate statistical analyses, is rivaled if not opposed by Thomson's Sampling Theory on the basis of statistical analyses no less elaborate.

The present position of the authors is admittedly tentative pending the outcome of experimental work under way in Professor Spearman's laboratory and elsewhere. Two quotations will perhaps state this position with greater effectiveness than would any paraphrasing which the reviewer might undertake. Restricted quotations are used under the assumption that the reader is acquainted with the 1921 edition. "The General Factor and the Specific Factors are each special cases of Group Factors. And therefore, although to postulate as large a General Factor as the correlations will allow is in one sense the simplest procedure by way of theory, in another and, we think, a wider sense, it is simpler to postulate only Group Factors, unless the approach to hierarchical order is so close that the Group Factors would be required to fulfil artificial and improbable conditions" (p. 197). "We must repeat that, while group factors may or may not be present, as long as the correlations are mainly positive a general factor may, of course, be postulated, and the controversy between us and Professor Spearman is not, and never was, as to the possibility of thus postulating a general factor, but as to the possibility of explaining all correlations thus without postulating any but the slightest group factors, and these very narrow in their action. Our position is that until the evidence is more

clear we shall continue to suspect that numerous and wide group factors are present" (p. 199).

The interest of Americans in this book is probably due as much to its contributions to statistical techniques as to concern over theories of intelligence. To be sure, as the title suggests, statistics is not the book's reason for being but references to the book in American writing as an authority on statistical procedures probably outnumber those made in discussions of theories of intelligence. And when the dispute over General Factors *versus* Specific Factors has waned the treatment of statistical method will quite rightly continue to attract students of psychology and education.

HENRY E. GARRETT. *Statistics in Psychology and Education*. New York: Longmans, Green and Co., 1926. 317 pp.

When the reviewer began teaching statistics, in 1921, there was such a dearth of elementary books which the average student could read understandingly that he did not use a text but supplemented the class lectures with references which he usually had to explain to the students who undertook to read them. Today, the situation is very different; there are many good books which present the elements of statistics for students in psychology, education, economics, and business. In several respects, Doctor Garrett's book is one of the best.

This book is not just another book on statistics. It is unique in its general plan of organization. There are only six chapters. Their titles are: I, the frequency distribution; II, graphic methods and the normal curve; III, the reliability of measures; IV, correlation; V, partial and multiple correlation; VI, some applications of statistical method and technique to tests and test results. Subsumed under Chapter I, are nine sub-heads: I, the tabulation of measures into a frequency distribution; II, measures of central tendency; III, measures of variability; IV, the short method of finding the average, AD, and SD; V, the comparison of groups; VI, the calculation of the percentiles in a frequency distribution; VII, when to use the different measures of central tendency and variability; VIII, summary of formulas for finding the measures of central tendency and variability; IX, illustrative problems. In like manner, each of the remaining chapters contains material which is usually presented in two or more chapters.

Herein lies the book's strength and its weakness. The plan will appeal to those who have studied statistics and wish a closely knit organization of the subject for review. On the other hand, so much that is new to the beginner is packed into so small a space that there is danger that the student will suffer from what may be termed statistical indigestion before he completes the first chapter. (The first chapter occupies just 58 pages.) Those who use the book as a class text will

find it necessary to go very slowly at first and provide a large amount of laboratory practice. It will also be necessary, in the opinion of the reviewer, to supplement the book rather freely with references and classroom explanations. Unfortunately, detailed references are not given with each of the chapters but are grouped in a general way at the close of the last chapter.

There are so few readable explanations of partial and multiple correlation available to the beginner that Doctor Garrett's excellent chapter on this subject should be appreciated. The treatment follows Yule in method and nomenclature, a welcome characteristic to the reviewer and others who have studied Yule but less fortunate in the eyes of those who have studied with Kelley. Detailed formulas are given for problems of three, four, and five variables. Formulas have been written so as to require the calculation of a minimum number of partial r's. This is an advantage in that some labor is saved but a disadvantage in that it will be difficult for the student to learn how to write the formulas for himself since the subscripts in the formulas are not written so as to reveal consistently a systematic plan. In the reviewer's opinion it would have been better to follow Yule a little more consistently. If a calculation machine and logarithms are used, partial r's are very easily and quickly calculated. The reviewer can calculate thirty partial r's in an hour and check all computations. The student of Doctor Garrett's book who undertakes to write out the formulas for a six-variable problem will probably find himself in difficulty.

The reviewer must take exception to the author's definition of the 0 and 100th percentiles. "These values are the lowest and highest scores, respectively, in the distribution" (p. 46). Since percentiles represent *points* and not distances on a scale, the 0 and 100th percentiles cannot be scores in the distribution. Or, to state it in another way, the 100th percentile is a point below which one hundred per cent of the cases lie. But one hundred per cent of the cases do not lie below the highest score. The 0 and 100th percentiles can not be calculated. If there are just 100 cases in a distribution the lowest score has a percentile value of 0.5 and the highest score, 99.5.

It is extremely difficult to keep such a book as Dr. Garrett's free from typographical errors. In this respect *Statistics in Psychology and Education* is well done. Errors occur (pages 35, 74, 76, 88, 103, 155, 239, 290, 294, 297 and elsewhere) but they are neither so numerous nor so serious as is often the case. The book has evidently been written and proofread with painstaking care.

Advanced students of psychology and education will do well to buy Dr. Garrett's book and keep it within easy reach. It is valuable. It will also be of value to teachers of these subjects in universities, colleges, and normal schools and to superintendents, principals, research workers

and others who are engaged with the solution of practical problems in the field.

R. L. MORTON,
Ohio University.

CHARLES EDWARD SKINNER, IRA MORRIS GAST AND HARLEY CLAY SKINNER. *Readings in Educational Psychology.* D. Appleton and Company, 1926. Pp. xxvii + 833.

This is a very valuable volume for reference in connection with courses in Educational Psychology. It covers quite thoroughly the contemporary literature on the topics conventionally included in such courses with additional chapters on Mental Hygiene and Interpretations in Psychology. It gives little attention to the history of the subject—a point perhaps in its favor—nor to the psychology of the school subjects. Although the majority of the references are to books, periodical literature has not been neglected. Indeed one of the great values of this book is this inclusion of studies hitherto isolated in magazines and journals. On the whole the volume is well balanced and free from obtrusive bias. An unfortunate exception to this occurs in the chapter on Interpretations in Psychology where one section is given to "Three Fundamental Errors of the Behaviorists" by Prince and another to "The Mechanistic versus the Personalistic Psychology as Applied to Religious Education" by Bentley in which Behaviorism is attacked, but no space at all is given to the behavioristic interpretation. Such an omission is surely not an oversight. It is hard, too, to justify the fact that the argument by Bentley is given three times as much space as any other interpretation.

The attempt to include a very large number of references has necessitated abbreviation and condensation which in most cases is unusually well done but which in some instances is carried too far. A single sentence taken entirely from its setting to cite the extremest example is hardly likely to be particularly instructive. It is a violation of the proprieties that in the discussion of intelligence as much space is given to the display of a test just devised by one of the authors as to the display of all other tests, individual and group, combined. In the opinion of the reviewer, it were better had the questions and problems at the end of chapters been omitted. A book of this sort can hardly be used successfully as a text and, of far greater importance, it is here that the points of view of the authors are inevitably emphasized.

In spite of the points criticized unfavorably, the book is an important one which every teacher of Educational Psychology will wish for his library.

L. L. BERNARD. *An Introduction to Social Psychology.* Henry Holt and Company, 1926. Pp. 651.

This is a very scholarly piece of work by one who has thoroughly familiarized himself with late developments in, not only psychology and sociology, but in neurology, biology and allied subjects as well. The book, therefore, represents a much more scientific and well balanced treatment than is usually found in one of similar title. From the point of view that "In a sense social psychology is central to all psychological and social science disciplines"—a view to which not all will agree—the author attempts to give a full and complete account of the foundations of collective behavior, the integration of personality in the psycho-social environment, the psycho-social environment and the organization of collective behavior. The author leans decidedly towards the behavioristic school of psychology and to that school of sociologists who believe that environment is of major importance as a determiner of individual and social behavior. Like many others, Bernard is unable at all times to free himself from verbal difficulties inherent in a new position. For example, he points out the anthropomorphisms of Ross on page 128 although, on page 65, he has himself said "The cortex is ever striving to intellectualize and acculturate man" and—lower down on the same page—"But the mind can project these conceptually . . ."

The presentation, as the author emphasizes, is complete in itself. It starts with the organic bases of behavior in various types of protoplasm and proceeds through an account of individual behavior to a study of the psycho-social environment. In the opinion of the reviewer, this is altogether too complete. It seems to him that psychology has advanced to a stage which justifies something besides elementary courses. There ought to be some sort of basic course the content of which may be assumed and not duplicated in so-called advanced ones. For a person who has pursued a good course in general psychology a very large portion of Bernard's book—as is equally true of most books in educational psychology, the psychology of advertising, etc.—would be wasteful duplication. It would seem strange if the texts in algebra, geometry and calculus should begin with drill exercises in the multiplication tables. Yet, no matter what phase of psychology is attempted by an author, he seems to think he must start at the beginning.

In another way, too, the text is too complete. In the preface the author states that it has been his intention to keep the presentation *on such a level* that the volume may be used as a *second* book in social theory, or as an *introduction to the first* course in sociology, economics, politics, and *more advanced* courses in history and literature, or in *advanced* courses in the social sciences! Does not this *level* appear somewhat futuristic?

D. A. WORCESTER,
Ohio University.

WILLIAM F. BOOK. *Learning How to Study and Work Effectively.* Boston, Ginn and Company, 1926. 475 pp.

This is a good book. The author, the head of the Department of Psychology at Indiana University and a pioneer in the experimental study of learning, whose classic study of the learning of typewriting has been quoted far and wide and furnished stimulus and suggestion to many workers, has, in the present volume attempted to outline the general principles as well as to give certain specific rules for personal efficiency in many forms of study and work.

Without attempting to discuss philosophic questions or to solve the perennial antinomies like that between the influence of heredity and that of environment, the author has collected and evaluated the results of observation and many experimental investigations. The text is made more clear and interesting by concrete illustrations collected from a wide range of reading and observation; and without ignoring the wisdom presented by the older psychologists like William James, and without failing to repeat the established rules for efficient management of business and industry, he has nevertheless presented the need of newer aims and higher ideals and the value of modern systems of administration, systematic habits, and the application of the teachings of scientific investigation.

Among the vast number of subjects represented on the basis of the author's extended scientific data are the following: the physiological and psychological factors that condition personal efficiency, the rôle of purposes and ideals in relation to work, the development of attention and will, types of decision and fatigue, the rôle of habit in learning, economic planning of work and the making and use of schedules of work, the conditions favorable for the most effective work, the development of interests and right attitudes toward work, and the different forms and conditions of learning. The way all this is based upon and illustrated by the author's experimental work is illustrated by the following paragraph:

"In learning to typewrite, for example, the learner must first take five steps in locating and striking each letter to be made on the machine. These five steps soon come to be fused into one so that each letter can be made directly on the machine as wanted. Later, certain letters are combined and reacted to as a group. In this stage the process of initiating and controlling the order and character of the movements required to make this group of letters can be controlled as a unit or handled just as easily as the single letter-making movements were controlled at a lower level of skill. Still later, a series of closely associated words comes to be handled in this unified way because a still higher order of habit has been established. Real efficiency in learning to study or to do other types of work consists therefore in the main of acquiring the ability to

make continually such improvements in one's methods of work as the learning proceeds" (pp. 451–452).

Finally the author adds a brief but valuable discussion of the need of right attitudes toward the principles of personal efficiency, the need of ability to do original work, and the danger of over standardization, pedantic orderliness and pseudo-efficiency. He emphasizes the need of cultivating one's own initiative and powers of creative thought and rightly emphasizes the dangers of set habits or standardized procedures, of mere conventional thinking, and closure of the door to human progress by slavery to habitual activity of any kind. He rightly says that "the chief source of advancement in learning and in life consists in increasing one's initiative, one's spontaneity, one's ability to do original and creative work."

The book is made the more valuable by the author's ability to control and correct his results by observation and verification in the classroom with the large numbers of students who for many years have come under his personal instruction.

The author emphasizes the importance of keeping oneself physically and mentally fit for work, and calls attention to the distraction and waste from ill health, fatigue and the like. The scientific studies cited and the examples given frequently illustrate the conditions favorable for the mental health, and attention is called to the significance of healthful mental attitudes toward one's work and the importance of success.

More extended discussion and illustration of these larger aspects of learning and the hygienic relations of the subject would seem desirable; but already the presentation has exhausted the limits of an ordinary text book; and the 475 pages of this volume give a splendid collection of carefully studied and extremely valuable material.

WILLIAM H. BURNHAM,
Worcester, Mass.

EDWARD K. STRONG. *The Psychology of Selling and Advertising.* McGraw-Hill Book Company, New York. Pp. 468.

In *The Psychology of Selling and Advertising* by Edward K. Strong we find offered several new and important changes in the general approach towards this subject.

In the first place, where heretofore emphasis in such a book has been placed primarily upon advertising, and especially the elemental factors of the advertisement, Strong discusses advertising from the selling viewpoint,—a method or means rather than the ultimate subject of the discussion. It must not be gathered, however, that this part of the book is sketchy, for it is far from that. In fact, discussions demonstrating the various appeals found in advertisements are some of the most vital sections of the entire book.

In the second place, a point that has been touched upon by other writers is developed by the author as the strongest theme in his work. This is the necessity of handling and developing the sales process from the buyer's point of view, rather than from the seller's. Strong has shown the importance of this both in the personal interview and through the medium of the advertisement.

A third point develops from the one above. By showing that all factors present in a sale can be analyzed into Want, Commodity, Trade Name (or Retail Store), Purchase, and Satisfaction (or Dissatisfaction), Strong claims that the former stereotypes of "attracting attention," "holding attention," "securing interest," etc., need no longer be emphasized if the campaign or sales process is laid out along lines that adhere to the principles of the always-existent sales formula,—keeping in mind the buyers wants, and working towards presenting a solution for them.

The book is divided into four major parts. Part I, *A General Survey*, of eight chapters, in which the most original phases of Strong's writing appear. Part II, *How Man Satisfies His Wants*. In this section of seven chapters, Strong follows the easiest road and puts the "fundamental wants" of man under the blanket-head of "native." It is to be regretted that while the book is so modern in every other detail, in this section Strong has ignored all controversy that has taken place in the last ten years, and lists and discusses human "wants" and appeals under the old nativistic classification.

Part III, *Principles of Selling and Advertising*, a section of eight chapters, is devoted to discussing the "buying formula," especially in terms of actual advertisements. Readers will find this section directly applicable to existing conditions in modern advertising and selling.

Part IV, *Strategy and Tactics of Selling*, of five chapters, is especially important in the continuing of the emphasis of the premise that the buyer must be the starting point in the sales process.

In general, the book is a real contribution to the field of Psychology in Selling and Advertising and has great worth not only as a college text book but also as a manual for the sales department of any firm.

S. L. Crawley,
Indiana University.

The Case For and Against Psychic Belief. A Symposium held at Clark University. Clark University, Publishers, 1927.

Since one cannot approach such a book as this with an open mind, the best one can do is to state his own prejudices and indicate how the book will appeal to those with similar bias. The reviewer does not pretend to intimate knowledge of metapsychic phenomena nor to an overwhelming interest in them. He is decidedly sceptical on *a priori* grounds as to the reality of any supranormal phenomena. Of course

the *a priori* must yield to opposed facts but it must not yield too easily. For we have to deal here not with a transcendental *a priori*-ism but with an empirical one. Our theories represent the compounded essence of too many patiently gathered facts; if they are to be abandoned for other facts, one wants to be sure that these are truly such. Now the complexity of the subject, the intrinsic difficulties of investigation (and many difficulties which to an outsider do not seem intrinsic), and especially the admitted stench of fraud all combine to make one very loath to grant that one is in the presence of assured fact. Such scepticism does not deny the value, nay the necessity of scientific investigation of metapsychic phenomena. The reviewer believes that from such investigation we may learn, indeed have already learned much concerning the psychology of testimony, of observation, of dissociation, and of the more obscure workings of certain kinds of minds.

On the other hand, one must insist that there are facts a-plenty which are baffling. In this volume Margaret Deland shows us how an intelligent but not scientifically trained mind reacts to the evidence amassed by psychic researchers. We have scientific generalizations which cover or which may cover most of the phenomena but there are unexplained residua. Mrs. Deland sees clearly enough that if these unexplained facts are accepted at their surface interpretation—indeed at the only interpretation we can at present envisage save that of faulty observation—a fundamental revision of our whole body of science is in order. She does not balk at this prospect; she insists that we *must* take account of these residua though the scientific heavens fall.

Well, taken absolutely these facts form a considerable mass; relative to the whole of phenomena they are almost infinitesimal. Astronomers do not give up the heliocentric theory because they cannot account for certain minor deviations in the orbit of Mercury. Nor should Mrs. Deland and her like forget that their spiritistic hypothesis in its turn would have unexplained residua. One is compelled to choose and one chooses to disregard a relatively small body of "facts" which one cannot as yet explain and make what progress one can along a well-worn highway. Let spiritists make of this what they may.

Those who share this viewpoint will get no sustenance from the papers of the two learned knights, Sir Oliver Lodge and Sir Arthur Conan Doyle. The predictive value of spirit-guided automatic script in the archeological researches of Mr. Bligh Bond is more impressive. But the address included in this volume does not go into details sufficiently to enable one to judge how rigid a control had been maintained.

Dr. L. R. G. Crandon, so long as he keeps to generalities about metapsychics, is interesting and plausible. That this account of the Margery mediumship should be a piece of special pleading was to be expected. Nevertheless it is a far more interesting account of what the inner circle claim for Margery than is to be found in *Margery-Harvard-Veritas*.

Unfortunately, here as elsewhere, the controversialists do not meet very squarely; vital points made by Dr. Walter F. Prince are ignored by Dr. Crandon and vice versa. But Dr. Prince's reply to the Margery group (reprinted from the *American Journal of Psychology*) leaves the present reviewer with few doubts as to the lack of good faith on the part of the Margery circle in the *Harvard-Veritas* pamphlet.

Mary Austin's contribution is a curious combination of anthropology, or rather folklore, and mysticism, written in a style which contrasts pleasantly with most of the others in the volume. She asks some exceedingly pertinent questions about the death-beliefs of primitive peoples and appeals for data about the death-experiences of modern man to help us read the riddle. It is well to have it pointed out to us that we know very little about what people think and feel when dying. For we can scarcely accept as evidence the mingled sentimentality and horror which make up the conventional account of dying moments.

So much for the decided "pro's." Wm. McDougall urges the value of psychical research as a university study. It affords a beautiful field for the application of scientific methods to actual data. (He forgets, as Europeans constantly do, that the function of our universities, even in graduate schools, is to instill facts.) And like Driesch, he calls attention to the philosophical importance of metapsychic facts. It is refreshing to find one who is so candid in admitting that his interest in certain types of phenomena has a metaphysical basis.

Hans Driesh entertains no slightest doubt about many of the metapsychic phenomena. But he ends with a plea for experimental control. If the medium tells us that there must be darkness, he suggests that we tell him we don't believe it and insist that a trial of light be made. (One wonders whether Driesch has ever tried this out on a medium; in popular parlance, "it listens good, but—.") "We must make the medium our object," he tells us, "and not be his."

Dr. W. F. Prince, besides his critical review of the Margery case, has an interesting resumé of what a critical believer in psycic research accepts. Dr. F. C. S. Schiller presents some logical aspects of psychic research. He attacks the demand for "absolutely cogent" evidence; none such is ever found in any field. He insists upon the logical value of the "bundle hypothesis" which is attacked by Coover and Jastrow. Despairing of many pragmatic tests as yet, he too appeals for more evidence and also for more elastic theorizing to account for the facts.

Dr. J. E. Coover rather threshes old straw in his demonstration of the frauds involved in a number of celebrated cases. Why prove what no one, not even Sir Arthur Conan Doyle, doubts, that there has been fraud in such cases as Palladino's, for example? His point is that such fraud justifies the severe incredulity of psychologists in cases where fraud is not demonstrated. A second point, to the reviewer more cogent, is the absence of, or the refusal to permit, investigation under conditions which make reliable report possible.

Dr. Gardner Murphy deals with telepathy as an experimental problem. If one approached the topic with a fresh mind, one might find the evidence which he presents impressive. As it is, those of us who have been inclined to look upon telepathy for various reasons as the most likely of metapsychic phenomena are apt to be rather disappointed in the lack of really conclusive facts. Murphy joins in the general demand for more and more experiments, but "an attempt to control all sources of error *at the beginning* is not only futile because of the impossibility of foreseeing all sources of error, but prejudicial to obtaining the kind of occurrences that one is out to observe. . . . . The procedure should be gradually tightened up so that one source of error after another is controlled."

The definitely antagonistic attitude is represented by Dr. J. Jastrow and Mr. Harry Houdini. The former attempts to show why men, especially scientists get led into the morasses of metapsychics and spiritism. "The one supreme condition is the experimenter and not the medium shall control the conditions. That indispensable requirement is always evaded, though seemingly accepted."

Houdini "was to have been a speaker in this symposium, but his untimely death prevented. . . . . The best existing statement of Mr. Houdini's convictions in this field are represented in the chapters . . . . reprinted from 'A Magician Among the Spirits.'" After exposing a few tricks, he devotes most of his attention, kindly but firmly, to Sir Arthur.

It has seemed worth while to review the book thus fully on the grounds that few readers of this JOURNAL need go beyond the review. Several of the contributors of the symposium insist in Murphy's phrase that "the task before the investigator today is not a polemic one." One would not so judge from this book. It may serve a useful purpose for the non-professional; psychologists will find it on the whole rather dry provender. One must add that the book contains more typographical errors than any other the reviewer remembers seeing in recent years.

HORACE B. ENGLISH,
Antioch College.

NORMAN FENTON. *Shell Shock and Its Aftermath.* C. V. Mosby Company, St. Louis, 1926. 173 pp. Price $3.00.

Dr. Fenton's recent book entitled, "Shell Shock and Its Aftermath", is a valuable contribution to the subject of the war neuroses. Dr. Fenton was one of the few men in the position to gather up all the facts pertaining to this medical-military problem and, with his first-hand information, as well as his keen psychological insight, humanize this data and compile it with statistical accuracy.

He has put in a hundred and sixty-seven pages what many authors might well have expanded to one thousand, yet, in being concise, he

has, in no way, sacrificed clarity. Although the final chapter has not been written on the subject of war neuroses, Dr. Fenton has thrown much light, not only on the path we have travelled, which has been only dimly perceived in its entirety, but has pointed out whither we are bound; and, I am sure, his contribution will stimulate further investigation by those to whom the medical care and re-habilitation of the veterans have been entrusted. As one closely associated with and deeply interested in the problem which the author has so adequately presented, I am greatly indebted for his efforts.

D. A. Thom,
Boston, Mass.

S. D. Porteus and Marjorie E. Babcock. *Temperament and Race.* Richard G. Badger, Boston, Mass. 364 pp. Price $3.00.

It has been recognized for many years that the different varieties of the human race exhibited different characteristics of temperament, intelligence, habits, and physical vitality and acquirements. It has also been admitted that history, political, religious, commercial and racial, has been determined to a greater or less extent by the diversity of these characteristics. In spite of this somewhat general cognizance of those important factors in the history and welfare of nations and races, little organized, scientific study of them was ever made until the last few years of this present century. And even at the present time the few facts available are chiefly physical and, to a much less extent, mental measurements and comparisons of two or more races.

The vastly important characteristic of temperament, what we might call the innate tendencies, of a race has been almost wholly disregarded due partly to failure to realize its importance but chiefly to the apparently hopeless complexity of the problem.

The work reported which forms the basis of the news expounded in this extremely interesting book was performed by the authors in what is probably the most desirable place in the world from an experimental point of view, Hawaii. The mixture of races combines every known race except the American Indian and several varieties of some of them.

Part I of the book is introductory, giving the reasons for the investigations, the importance of temperament in the race, and the methods to be used.

Part II gives a very interesting and valuable history of the different races present in Hawaii. This part alone would justify anyone in spending the time to read it carefully, leading as it does to the heart of of the problem of over-population in certain countries, with its attendant dangers of economic and racial imperialism. This part is extremely informative to anyone interested in the origin of such a diversity of peoples as occur in this small group of islands.

Part III is a social analysis of the different races and gives the results of the individual temperament of the race. The different races are rated according to the strength or weakness of certain social traits. Some of the social traits listed are lack of planning ability, impulsiveness, irresolution, etc., and their opposites. From these it can be seen that the method while rather unique and recent in scientific investigation, goes to the very center of the cause of racial difference and superiority or inferiority.

A racial efficiency index is worked out from the scores in the different social traits. It will be of interest to any reader to see the results of this ranking to find that the Japanese and Chinese are at the top.

Thus in the light of scientific studies, incomplete and inconclusive though they may be, we are beginning to see that there are few bases for our exalted opinion of the white races. Again in social adjustment as measured by lack of criminality, business initiative, success in professions and trades, etc., we find many surprises.

Part IV deals with the brain development of the different races and gives some very illuminating facts and conclusions concerning the cranial capacity at different ages for both sexes of Anglo-Saxon, Japanese and Chinese children. The Japanese are shown to have a cranial capacity greater than the Chinese, and equal to whites of the same social standing. The history of the races in Hawaii is of importance here in classifying the children into the proper social groups.

Part V gives facts relative to the mentality of the different races. Binet tests and Army tests were made. In these tests the Chinese compared very favorably with Northern Europeans and Americans, and excel considerably Southern Europeans. The authors very carefully point out however that these tests are only a measure of school intelligence and not of racial intelligence. In order to measure racial intelligence the tests for social traits and adaptability must be included.

Parts VI and VII are given to a discussion of the importance of exact knowledge concerning the relation of temperament to race and to racial futures. Strength of racial character is needed to safeguard a nation or a race rather than military fortifications. The Japanese are shown to be superior to all other groups in planning capacity, resolution and self-control. New ideas of behavior consistent with character must be adopted to conserve whatever superiorities of race we may possess.

EMMETT ROWLES,
Ohio University.

NEW BOOKS AND PAMPHLETS RECEIVED*

Books and pamphlets for review should be sent to James P. Porter, Department of Psychology, Ohio University, Athens, Ohio.

Abilities of Man, The. C. SPEARMAN. Macmillan Company. $4.50. 415 pp.

Characteristic Differences in Bright and Dull Pupils. HARRY J. BAKER. Public School Publishing Company, Bloomington, Ill. 118 pp.

Educational Psychology—Revised Edition. DANIEL STARCH. Macmillan Company, New York, 1927. 568 pp.

Fourteen Is Too Early. RAYMOND G. FULLER. National Child Labor Committee, 215 Fourth Ave., New York City. 40 pp.

Investigations in the Hygiene of Reading. JAMES HERBERT BLACKHURST. Warwick & York, Baltimore, Md. 1927. 63 pp.

Learning and Living in College. CARL E. SEASHORE. University of Iowa Studies, University of Iowa, Iowa City, Iowa. $1.00. 124 pp.

Mind and Personality. WILLIAM BROWN. G. P. Putnam's Sons, New York, 1927. 356 pp.

Psychology By Experiment. LINUS WARD KLINE AND FRANCES LITTLETON KLINE. Ginn & Company, New York. 339 pp.

Psychology of Mental Disorders. ABRAHAM MYERSON. Macmillan Company, 1927. $1.40. 134 pp.

The Psychology of Social Institutions. CHARLES HUBBARD JUDD. Macmillan Company, New York. 346 pp.

Psychology—The Science of Mental Activity. FREDERICK H. LUND. A. G. Seiler, New York, 1927. $2.50. 488 pp.

Research Adventures in University Teaching. S. L. PRESSEY, D. A. WORCESTER, J. M. FERGUSON, J. A. SEATON, L. C. PRESSEY, O. R. CHAMBERS, H. J. ARNOLD, H. C. MARTIN. Public School Publishing Company, Bloomington, Ill. $1.50. 152 pp.

Sex Differences in School Children. EDWARD ANDREWS LINCOLN. Warwick & York, Baltimore, Md. 1927. 189 pp.

A Statistical Study of Certain Aspects of the Time Factor in Intelligence. FRED C. WALTERS. Teachers College, Columbia University, Bureau of Publications. 82 pp.

A Student's Dictionary of Psychological Terms. HORACE B. ENGLISH. Antioch College, Yellow Springs, Ohio.

Your Money's Worth. STUART CHASE AND F. J. SCHLINK. Macmillan Company, 1927. $2.00. 285 pp.

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* Mention here does not preclude further comment.

Directory of American Psychological Periodicals

American Journal of Psychology—Ithaca, N. Y.; Morrill Hall.
Subscription $6.50. 600 pages annually. Edited by M. F. Washburn, K. M. Dallenbach, M. Bentley, E. G. Boring. Quarterly. General and experimental psychology. Founded 1887.

The Pedagogical Seminary and Journal of Genetic Psychology—Worcester, Mass.: Clark University. Subscription $7. 700 pages annually. Edited by Carl Murchison and an international co-operating board. Quarterly. Child behavior, differential and genetic psychology. Founded 1891.

Psychological Review—Princeton, N. J.: Psychological Review Company.
Subscription $4.25. 480 pages annually.
Bi-monthly. General. Founded 1894. Edited by John B. Watson.

Psychological Bulletin—Princeton, N. J.: Psychological Review Company.
Subscription $5. 720 pages annually. Psychological literature.
Monthly. Founded 1904. Edited by Samuel W. Fernberger.

Psychological Monographs—Princeton, N. J.: Psychological Review Company.
Subscription $5.50 per vol. 500 pp. Founded 1895. Ed. by Shepherd I. Franz.
Published without fixed dates, each issue one or more researches.

Psychological Index—Princeton, N. J.: Psychological Review Company.
Subscription $1.50. 200 pp. Founded 1895. Edited by Walter S. Hunter.
An annual bibliography of psychological literature.

Journal of Philosophy—New York: 515 W. 116th St.
Subscription $4. 728 pages per volume. Founded 1904.
Bi-weekly. Edited by F. J. E. Woodbridge and Wendell T. Bush.

Archives of Psychology—Sub-Station 84, N. Y.: Archives of Psychology.
Subscription $5. 500 pp. ann. Founded 1906. Ed. by R. S. Woodworth.
Published without fixed dates, each number a single experimental study.

Journal of Abnormal Psychology and Social Psychology—Albany, N. Y.
Subscription $5. Boyd Printing and Publishing Co. Edited by Morton Prince in co-operation with Floyd H. Allport.
Quarterly. 432 pages annually. Founded 1906. Abnormal and Social.

Psychological Clinic—Philadelphia: Psychological Clinic Press.
Subscription $2.50. 288 pages. Ed. by Lightner Witmer. Founded 1907.
Without fixed dates (9 numbers). Orthogenics, psychology, hygiene.

Training School Bulletin—Vineland, N. J.: The Training School.
Subscription $1. 160 pp. ann. Ed. by E. R. Johnstone. Founded 1904.
Monthly (10 numbers). Psychology and training of defectives.

Comparative Psychology Monographs—Baltimore: Williams & Wilkins Co.
Subscription $5. 500 pages per volume. Edited by W. S. Hunter.
Published without fixed dates, each number a single research.

Psychoanalytic Review—Washington, D. C.: 3617 10th St., N. W.
Subscription $6. 500 pages annually. Psychoanalysis.
Quarterly. Founded 1913. Ed. by W. A. White and S. E. Jelliffe.

Journal of Experimental Psychology—Princeton, N. J.
Psychological Review Company. 480 pages annually. Experimental.
Subscription $4.25. Founded 1916. Bi-monthly. Ed. by Madison Bentley.

Journal of Applied Psychology—Baltimore: Williams & Wilkins Co.
Subscription $5. 400 pages annually. Founded 1917.
Quarterly. Edited by James P. Porter and William F. Book.

Journal of Comparative Psychology—Baltimore: Williams & Wilkins Co.
Subscription $5. 500 pages annually. Founded 1921.
Bi-monthly. Edited by Knight Dunlap and Robert M. Yerkes.

Journal of Personnel Research—Baltimore: Williams & Wilkins Co.
Subscription $5.00. 500 pages approximately. Edited by Walter V. Bingham, Lewis L. Thurstone, and Clarence S. Yoakem. Monthly.

Genetic Psychology Monographs—Worcester, Mass.: Clark University.
Subscription $7. 600 pages annually. Edited by Carl Murchison and an international cooperating board.
Bi-monthly from January, 1926. Each number one complete research. Child behavior, differential and genetic psychology. Founded 1925.

Psychological Abstracts—Princeton, N. J.
Subscription $6.00. 600 pages ann. Edited by W. S. Hunter. Monthly.
Abstracts of psychological literature. Founded 1927.